MOTOR, VISUAL AND APPLIED RHYTHMS

BY

JAMES BURT MINER, M.S., LL.B.,

University Fellow in Psychology, Columbia University

SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY
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PART I. DESCRIPTION AND EXPLANATION OF RHYTHM.

A. From the Psychological Side.

The attempt to explain any state of consciousness brings with it first the necessity of describing the experience in mental terms. If we can make this explanation elementary enough we shall at once have pointed out the physiological correlate. In so far, then, as we can suggest a physical basis on which this bodily accompaniment may arise, we explain the phenomenon scientifically. In the first part of this paper I shall try to follow through such a course for rhythm. I was led to undertake it by experimentally demonstrating certain muscular accompaniments of rhythms, which I shall describe in connection with the physiological discussion. At the outset it becomes me to recognize the difficulties which lie in the way of any rounded explanation. I am encouraged, however, by the fact that the direction for future progress is often most clearly seen after an attempt to harmonize our knowledge on any point into a comprehensive whole.

Primary rhythm has been taken to mean the regular succession of like events, while secondary rhythm brings in the factor of grouping among these events. We shall use rhythm only in the sense of rhythmic grouping. To the psychologist a series of sounds takes on a new interest as soon as they are heard in groups. The grouping to which I refer is a true rhythmic experience, something more than counting the sensations in groups. There is a feeling of the group together. The group is 'felt,' so to speak, rather than counted. Each group is experienced as a sort of whole, which is compared with other wholes. The groups have their limitations and, like other perceptions, they are accompanied by an affective tone. In artistic rhythms the grouping is embellished by ideational associates. In this discussion we are to treat of only the simplest feeling of

rhythm and try to explain that. We seek the fundamental fact that is common to rhythm, whether it be experienced in poetry, music, movement, or in a mere succession of noises. This fundamental fact, which sets rhythm off from other impressions, is that the groups are felt as units. Meumann, who has written the classical German monograph on rhythm, expresses this as the 'Zusammenfassung der Eindrücke.' He says: "The subjective holding together of the impressions in a whole is inseparably bound up with the simplest cases of rhythmic perception."[1] Bolton, in the first empirical study directed specifically toward rhythm, speaks of the sounds running together to form organic groups.[2] He lays much stress on the group appearing as a unit.[3] Squire speaks of the 'unitary character' of the group and of its 'unity for perception.'[4] Titchener calls this the 'essential thing in the perception of rhythm.'[5] We recognize the factor clearly in the case of a series of like sounds at uniform intervals which are rhythmically experienced. Explanations of rhythm have generally been directed toward the unitary character of the group. It is to this factor that I shall especially attend.

As soon as we recognize that rhythm brings into consciousness a group feeling or 'Zusammenfassung,' which is not present in the objective stimuli, we seek at once some conscious process on which this can rest. When the rhythm is experienced from an objectively uniform series, either the sensations themselves must become actually united in time or by real changes in them; or else they must appear united by reason of some other accompanying element in consciousness which adds the group feeling. This element would, as it were, provide the thread on which the sensations from the external stimuli are strung out in groups. I shall contend that the evidence points to this latter suggestion; that feeling the groups to be units is an illusion due to the presence of movement or strain sensations along with the sensations that are grouped; that these sensa-

[1] *Philos. Stud.*, X., 271.

[2] *Amer. J. of Psychol.*, VI., 204.

[3] *Op. cit.*, p. 213.

[4] 'A Genetic Study of Rhythm,' p. 86.

[5] 'Experimental Psychol., Qualitative, Students' Manual,' p. 174.

tions arise from movement or tension started reflexly in the muscles by the external stimuli. These kinæsthetic sensations are interpreted as an apparent change in a uniform objective series. The groups appear to be separated either by a longer interval or by a regularly recurring accent or both. In any case a kinæsthetic sensation within the group gives us the uniting element for consciousness. I shall attempt to develop this view rather completely and point out how other explanations seem to me to be inadequate. In connection with the treatment of the physiological accompaniments of rhythm, I shall present some new empirical evidence supporting this kinæsthetic explanation.

We can most conveniently discuss the questions involved by taking up first three other lines of explanation that have been offered. These ascribe the unitary group feeling (1) to respiration or some other regular organic rhythm; (2) to attention; (3) to expectation and satisfaction.

Suggested Explanations.

1. Regular Organic Rhythms.

Perhaps the most natural factor to select, as providing something in consciousness on which separate sensations from the external world could be strung in groups, would be the sensations coming from one of the organic rhythms. The fact that the rhythmic group repeats itself in a uniform time requires that the element for grouping *may* come regularly. Pulse and respiration at once suggest themselves. The changing feeling due to breathing, for example, would provide a grouping element not found in the external stimuli. It might be completely adequate provided its wave-length corresponded to that of the rhythmic group. Just here, it seems to me, is where any organic rhythm which regularly continues in us at all times fails as an explanation of rhythm. Its period would be practically a constant. A brief consideration of the voluntary conception of rhythm shows that organic rhythms can never adequately explain all rhythmic impressions. The fallacy lies in supposing that any regular organic pulse of movement could be so

changed that its period would agree with whatever length of group was chosen. We can experience a rhythmic group whether the stimuli be .1 sec. or 4 sec. apart.[1] This range is far beyond that of any organic rhythm. Moreover, there are no gaps in the rhythmic experience within these limits, as we would have to expect whenever the rhythmic bodily change did not exactly coincide with the period of the group. By organic rhythms in this discussion I mean, of course, the regular rhythmic processes constantly going on in our bodies whether we are in the presence of serial stimuli from the outside or not.

Squire recognizes that the organic rhythms cannot be regarded as the source of the perception of rhythm.[2] As for respiration, she found that in reading syllables in different rhythms there was only one form, the trochee, and a single case of iambic reading, in which the respiration curve corresponded to a complete group.[3] Meumann criticises the explanation of rhythm by general bodily rhythms. He observes further in this connection that the feeling we get from the constant rhythms is too indefinite to provide a grouping factor. He regards it as inconceivable that 'the uncertain sensation elements coming from the organic rhythms can produce the ordering of the sound impressions in rhythms.'[4] He would doubt, moreover, that the course of these rhythms of the body could be shown to run parallel with the rhythmic impression.[5]

For the most part the suggestion of the feeling from the bodily rhythms has been only tentatively or indirectly mentioned by investigators to explain the binding of separate sensations into groups. The tendency to use this explanation, however, is strong and appears in various forms. It seems to have been emphasized by the work of experimenters like Mentz and Leumann, who have shown that rhythmic impressions are accompanied by changes in the pulse and breathing. Leumann found that, as the rate of scansion increased, the heart beat accelerated and the breathing changed.[6] Mentz comes nearer to

[1] Wundt, 'Human and Animal Psychol.,' Titchener's trans., p. 263.
[2] *Op. cit.*, p. 85.
[3] *Op. cit.*, p. 74.
[4] *Philos. Stud.*, X., 272.
[5] *Op. cit.*, p. 254.
[6] *Philos. Stud.*, V., 618.

offering respiration as an explanation of rhythm. He states that 'a simple metronome beat, as well as the accented beat of a given measure, produced through the direct innervation of the breathing an impulse to the beginning of an inspiration or expiration.' With a subjective rhythm he states that there is also this 'frequent falling together of the accented beat and the respiration summit or valley.'[1] It is to be noted that he speaks of a 'frequent falling together,' and that is what his work shows. However, this occurred when the subject was very likely choosing a rate best adapted to breathing. It would not hold for every period of grouping. Mentz even gives schematic curves showing three and four metronome beats arranged on a respiration wave. Very curiously, under his view, the respiration wave seems to include less beats when the metronome is running at a higher rate.[2] Such investigations are important as showing the effect of rhythms on the bodily conditions. They indicate that a respiration wave may, in some cases, be the basis of grouping; but they leave out of account the difficulties arising if it is attempted to show that the respiration is always the basis of the group feeling. It is far from my intention to claim that rhythms do not affect the organism. This is very different, however, from supposing that the bodily rhythms explain the rhythmic impression.

Organic rhythms are more generally used to explain the preference for groups of a certain period. This seems to be more justifiable. Seashore, for example, in one of his experiments with the free tapping of rhythms, suggests that the length of the group 'seems as a rule' to fall within the range of the respiration and pulse periods of all the subjects tested, or multiples or divisors of these periods.[3] This is without regard to the period of the particular subject beating the rhythm. I shall treat of this question further in connection with the limitations of grouping.

The fallacy of choosing an organic rhythm to give the unitary feeling to a group is the same whether respiration, the

[1] *Philos. Stud.*, XI., 123.
[2] *Op. cit.*, p. 121.
[3] *University of Iowa Studies in Psychol.*, II., 72.

pulse or some other regular rhythm is chosen. Fatigue, for example, is supposed to rise and fall, and might explain the tendency to favor a certain length of group. But a condition of fatigue could not correlate in consciousness with the grouping of a series of stimuli in twos whether they be .1 or 1 sec. apart. A constant brain pulse or rhythm of nervous discharge, if there is any such regular rhythm taking place, would present the same difficulty. We are not justified in supposing that any constant rhythm could be changed and distorted to provide a grouping accompaniment of various periods. Meumann suggests something like a brain pulse in connection with his attention explanation, which I shall treat under that head. MacDougall also allows for such a possible explanation of rhythm, in addition to the factor of kinæsthetic sensations which he mentions. He says "We may conceive a periodical facilitation and inhibition of nervous activity to arise from the relation between the periodicity of its own rhythm of functioning and certain intervals in the objective series of stimulations. If such a physiological rhythm appears in the functioning of the central nervous system, a periodic increase and decrease should occur in the intensity of the sensations coördinated with a series of unchanging stimulations, according as the elements were coördinated with positive or negative phases of the nervous activity."[1]

He is speaking here, I take it, of a continued rhythm of action which the nervous mechanism may show in some constant way. If there were a regular increase and decrease in the intensity of the current, either from the sense organ or in the brain, we should have a real change in sensations. This would be fundamentally different from the illusion of change on account of kinæsthetic accompaniments, which I suggest. The supposition of such a brain pulse, with the idea that it is comparatively constant, has the same inadequacy as other organic rhythms.

2. Attention.

There is considerable confusion in the use of the word attention by those writers who would ascribe the unitary feeling of the group to it. The wave of attention may mean the fact

[1] *Psychol. Rev.*, IX., 465.

that there is a more or less regular rise and fall in the apparent intensity of a sensation which is continually attended to. We find this in the appearance and disappearance of sensations near the threshold. This wave of attention would probably be explained as correlating with a regular rhythm of the brain or sense organ, perhaps with fatigue and restoration. It would then be open to the objection I have raised to a constant bodily rhythm. Again, attention is apparently used to describe something different from the 'wave of fatigue.' Greater attention is used as a name for the fact that certain sensations in a series appear more distinct to us. This would be a correct use of the term, no doubt, but it would not provide any other factor than the original sensations themselves to produce the grouping effect.

Meumann refers the grouping to straining of the attention. "The general psychological facts," he says, "to which the sensory rhythm field is referred for a basis of explanation, are certain relations of attention, strain periods and the changing extent of the same."[1] Again, he says that the entire phenomenon may be regarded as "an unlike energy division of the attention. * * * When the separate beats follow each other at the rapidity of .3 sec., it is impossible for us to employ the same energy of attention throughout. However, if we alternately heed and do not heed a like number of impressions, we attain the double purpose of being able to follow the progress and the temporal succession correctly, *i. e.*, without losing an impression."[2] With Meumann accent is an expression of the greatest employment of attention. Rhythm may certainly be described as Meumann has here; but we must wonder if, in doing so, we are providing any factor for grouping except the name attention. Meumann surely does not hold that it is the straining of attention which is felt as coördinate with the groups. Attention is not something psychical which we feel. That which Meumann means by attention seems to be brought out somewhat better when we consider his physiological correlate for this condition. He says that the facts of energy distribution of attention are 'to be

[1] *Philos. Stud.*, X., 429.
[2] *Op. cit.*, X., 304.

given a point to hold to in the facts of central energy change in successive perception, of central adaptation and automization.'[1] The central energy change seems very like a brain pulse, although it is apparent that Meumann is struggling away from such a regular rhythm when he refers to 'central adaptation.' This might be supposed not to be a constant process, but one dependent upon the incoming stimuli.

Squire and Bolton also use the attention to explain the group feeling. Squire says: "One group corresponds to one pulse of attention."[2] Bolton's view is that 'only one undivided state of consciousness may arise during each pulse of attention,' and further that 'the number of objects which can be grasped in that state must form an organic unity or be presented as a single object — have the appearance of a unit.'[3] Again, he says:

> "This rhythmical grouping was a series of efforts to attend to the sound. The grouping results from a sequence of acts of attention. When the attention is directed to the sensation it lays hold of the first impression with great force and makes it the sole object of consciousness. If this were the only sound, the attention would turn to something else; but as succeeding impressions follow before the first wave of attention has subsided, they are seized upon with less force than the first impression and are subordinated to it in different degrees according to the strength of the apperceptive act. Subsequent waves of attention follow the same process as long as the will directs the attention to the phenomena. The attention accommodates itself to a certain number of impressions, which fall easily within the period of a wave, providing there is no objective difference in the impressions. If there is a regular recurrent difference this becomes the signal for a new act of attention, providing only that the span does not exceed or fall much under the normal period of a wave."[4]

Here we seem to have sometimes a description of rhythm in terms of attention and then again a coördinate for the group in some regular bodily change, which is called here the wave of attention. The quotations from these writers illustrate in what way attention has been used in connection with rhythm. They indicate, it seems to me, that we must look beyond attention if we are to find any real factor in grouping for which we can seek a physiological correlate. It will not do to say that attention does the grouping. Attention does nothing in con-

[1] *Op. cit.*, p. 429.
[2] *Op. cit.*, p. 86.
[3] *Amer. J. of Psychol.*, VI., 213.
[4] *Op. cit.*, p. 211.

sciousness. Neither is it an independent conscious experience which we feel. In describing rhythm in terms of attention we are not providing any element which would bring the feeling of grouping into the series, or else we seem to be providing another regular organic rhythm, which would be as inadequate as respiration to give the rhythmic impression in various voluntary groupings.

3. Expectation and Satisfaction.

Among the German investigators Wundt has set a type for the description of rhythm which is quite generally followed. With him rhythm is an emotion arising out of the regular alternation of states of expectation and satisfaction. 'Der Rhythmus ist ein Affekt, bei dem Erwartung und Befriedigung zusammenfallen.'[1] Smith agrees in general with this description of the experience.[2] It is apparent that we have here a different conscious element on which we may say that the feeling of unity in the group rests. Moreover, this feeling of expectation has not the objection which I have raised to the sensations from a regular organic rhythm. It has no constant period, but changes with the rhythmic impression. Two difficulties, however, are at once presented when choosing expectation to explain the group feeling. In the first place expectation is a rather complex experience and we should seek to describe rhythm in the simplest terms possible. Again, we are at once in a quandary when we seek to correlate expectation with a physiological condition. The primitive feeling of rhythm may mount, as it increases in content, to an emotion of expectation. It seems better, however, to keep our description of the elements of rhythm in terms of sensation, where that is possible. If we reduce this feeling of expectation to sensation terms, I take it that we have essentially the kinæsthetic sensations of tension and relaxation, or of waving movement. This would remove the objection against the complexity of a feeling of expectation and would at once indicate the physiological correlate. Wundt's explanation has the advantage of suggesting an element in consciousness which would coincide with the group, while the feeling from regular bodily rhythms would not.

[1] 'Menschen und Thierseele,' p. 311.
[2] *Philos. Stud.*, XVI., 291 ff.

In connection with Wundt's description we may well consider the emphatic objection of Squire to speaking of rhythm as an emotion. Titchener had raised the question whether rhythm should be classed as a perception or as an emotion, but had not answered it.[1] The argument against describing rhythm as an emotion has two aspects which may be considered independently. As a formal matter we may agree that it is not well to class rhythm as a pure emotion; but in the second place, while doing this, we do not have to follow Squire in describing it as a pure perception devoid of affective tone. If we describe rhythm as we do similar experiences, we may very well say that it is a perception accompanied by a characteristic emotional tone. Squire's objections are well formulated and should be stated at length, for in part they conflict with the thesis which I am offering. She says:

"No explanation which makes the affective elements fundamental to rhythm can be satisfactory. (1) Introspection shows that the rhythmical grouping can occur in a perfectly indifferent conscious state. This was noted by the subjects of Bolton and Smith as well as our own. (2) Feelings become blunted by repetition. If, then, rhythm originates in partial feelings, which Wundt makes intermediaries in each and every group, we should expect a gradual weakening of the affective tone of rhythm with prolongation of rhythm; but this is not true. As a rule the affective tone generally increases for a considerable length of time, especially when organic co-vibrations are set up. (3) The feeling when present does not consist of a series of contrasted feelings, such as any theory which makes feeling the intermediary of grouping must presume; on the contrary it runs a comparatively unbroken course of either gradually increasing pleasantness, or, when reversed, of gradually decreasing pleasantness. The contrast brought out in grouping is ideational in source. (4) No explanation of rhythm which goes out from the feeling side can successfully explain the limitations which all groupings show (the limitations to two and three groupings and their compounds). (5) The gradual growth of rhythmical ability and rhythmical perception can be accounted for only on the grounds of its perceptual nature. (6) The characteristics of the affective curve are not present in the rhythmic curve taken by the pneumograph. The curve, as has been shown, is that characteristic of an attentive state. (7) Furthermore all the phenomena of rhythm can be explained by the facts of perception."[2]

Squire was not the first investigator to object to describing rhythm as expectation. Ebhardt found no feeling of expectation and satisfaction among his subjects, but prefers to speak of a period of strained attention followed by a feeling of empti-

[1] 'Experimental Psychology, Qualitative, Instructor's Manual,' p. 352.

[2] *Op. cit.*, p. 96.

ness.[1] When discarding the factor of expectation on which to base grouping, it is interesting to note that both these writers substitute attention. Introspection, Squire believes, does not show the presence of expectation or of 'alternate feelings of strain and relief.' "What we do find by introspection is a constant forward direction of the attention."[2] If these writers were to state their introspection as to the presence of attention in some non-attention terms, it is difficult to see how they would avoid using kinæsthetic strain sensations. The important feature of this criticism of Squire seems to me to be its bearing on the general psychological method of description. Do we ever have a perception that is perfectly indifferent in feeling tone? What do we mean by indifference? It is commonly assumed that all states of consciousness have some feeling attribute. If rhythm does not we should revise our hypothesis. We should probably agree that whenever the subject is active he is experiencing some change of emotion. It is impossible to conceive an absolutely passive individual. Changes in the body are reflexly started by every incoming stimulus. What then is this feeling of indifference of which we speak? Shall we say that during indifference bodily changes are not affecting us, or is it better to regard them as affecting our conscious content, but not separately perceived. The latter view seems to me in better form. When tension or movement is not specifically felt in rhythm I should regard them as still in the background of consciousness, really affecting the conscious state somewhat as James would say it is affected by its fringe. The difficulty ~~in~~ introspecting the impression of rhythm beyond ~~mere terms~~ of attention easily accounts for subjects not all noting tension or movement sensations. With trained introspectors I have never found any difficulty in tracing what are commonly called strained periods of attention to actual kinæsthetic sensations. Two other points of Squire's should not be passed without a word. What she probably means by the respiration curve not showing affective attributes, is that it did not show the characteristics of expectation as outlined by Lehmann and noted by her on a previous

[1] *Ztsch. f. Psychol.*, XVIII., 191.
[2] *Op. cit.*, p. 86.

page.[1] The claim that emotions always decrease with repetition does not seem to me sound. Melancholy, for example, grows by its repetition until it overpowers us. In conclusion, I would repeat my objection to explaining rhythm by attention when that term may be resolved into kinæsthetic sensations, as I believe it can be here. Furthermore, it seems to me just as much out of the usual order to describe any mental experience started by objective stimulation as a pure perception, as to regard it as a pure emotion. I prefer, therefore, as I have stated, to call rhythm a perception with an affective tone. In doing so I believe we keep closer to the best psychological methods.

4. Kinæsthetic Sensations.

Now that we have considered some of the objections that may be urged to using attention, expectation or sensations from the constant organic rhythms as the factor explaining the group feeling in rhythm, we return to the thesis which I offered previously as adequately explaining the rhythmic experience from a psychological point of view. The thesis is this. The impression of rhythm, or the group feeling in connection with serial stimuli, arises from the coördinate presence in consciousness of a wave of kinæsthetic sensations, due either to a movement or tension of the muscles. The perception of grouping is thus not the direct result of the sensations from the outer world, but is read into the objective series. The 'outer' sensations are not actually grouped, but appear to be grouped. Rhythm thus arises as a time or intensity illusion in much the same way as we speak of space illusions arising from eye movements. This hypothesis is not entirely new to writers on rhythm. Indeed, in view of the almost universal observation of movements accompanying the perception of rhythm, it is strange that it has not been more completely developed before. Almost the first thing that we notice in the presence of a series of sounds recurring at short intervals is this tendency to move. It is felt either as a tension of the muscles or as an actual contraction of them. Before taking up the views of other investigators on kinæsthetic sensations let me repeat with emphasis that the selection of

[1] *Op. cit.*, p. 95.

tension and movement sensations, as providing the conscious factor for the feeling of unity in the rhythmic group, is essentially different from the selection of sensations coming from the diaphragm alone, from any constant organic rhythm, or from a general pulse of bodily feeling. Respiration may at times provide these kinæsthetic sensations for rhythm, but frequently does not do so. It is doubtful if any other regular organic rhythm ever provides the factor for grouping to consciousness. One of the chief characteristics which I would urge in favor of tension and movement sensations is that we are not bound down to any constant rhythm which varies with difficulty and within narrow limits. Movements or tensions in the head, arms, feet, or muscles anywhere may give the conscious element for grouping. They alone do not, of course, explain the limitations of grouping, which I shall take up in connection with the physiology of rhythm. It is in that connection that we have need of constant rhythms to provide a horizon and zenith for our rhythmical impression.

Previous investigators have quite generally referred to the kinæsthetic accompaniments of rhythm. Some of them have approached very near the thesis defended in this paper. Bolton was the first to suggest that 'if the sounds become too rapid to find expression in muscular contractions of any kind, they can be no longer separated from one another as single impressions,' and hence not grouped.[1] Further he says: "The change from one state of consciousness to another is represented by the reversal of a muscular movement. If between two impressions there is not sufficient time or time equal to the reversal of motion in a member, there is not consciousness of an interval between the impressions."[2] Since he regards a rhythmic group as 'one object of consciousness,'[3] we have only to suppose a reversal of muscular activity between the groups to place the correlation of rhythm on a muscular basis. In another place Bolton says: "Each impression as it enters into consciousness tends to find expression in a muscular movement, but the intensive changes in the series of impressions produce corresponding changes in

[1] *Amer. J. of Psychol.*, VI., 220.
[2] *Op. cit.*, p. 221.
[3] *Op. cit.*, p. 155.

the intensity of sensations, which must find expression in different degrees of muscular activity."[1] This traces a rather complete connection between an objectively accented series and the sensations from the muscles. It is incomplete so far as providing a muscular coördinate for grouping from an objectively uniform series. I would contend that the muscular tension or movement in the case of such a series also provides the grouping element. Bolton also makes much of the presence of involuntary movements during rhythmization, to which I shall again refer in connection with my own experiments.

With Wundt we have 'inner tactual sensations' which correspond to kinæsthetic sensations, as the element with which to explain accentuation in subjective rhythms. He says:

> "The primary cause of the actuation of a particular impression is always to be found in the increase in the intensity of the preceding and concomitant feelings and inner tactual sensations of movements. This increase in the intensity of the subjective elements is then carried over to the objective impression, and makes the latter also seem more intense. The strengthening of the subjective elements may be voluntary, when the tension of the muscles which produce inner tactual sensations is voluntarily intensified, thus producing a corresponding intensification in the feeling of expectation. Or the strengthening of the subjective elements may be involuntary, when a grouping of the elements of the temporal idea is brought about as an immediate consequence of the fluctuations in sensation and feeling that take place during the effort to include as many factors as possible in the percept."[2]

Wundt thus recognizes a carrying over of the kinæsthetic sensations to a changed appearance in the objective series, and also the involuntary strengthening of elements in a group by concomitant feelings. 'Strain and relaxation,' which Wundt regards as one of his three fundamental 'dimensions of feeling,' are certainly very clearly connected with the muscles. Rhythm is given by him as the chief illustration of strain and relaxation feelings, although he indicates that rhythm may also show the presence of his other affective dimensions, 'pleasure and pain' and 'rousing and subduing.'[3] There is probably only a verbal difference between Wundt's 'strain' feeling and my muscular 'tension' sensations which accompany rhythm. In connection with accent and with these strain feelings it is

[1] *Amer. J. of Psychol.*, VI., 235.
[2] 'Outlines of Psychology,' Judd trans p. 167.
[3] *Op. cit.*, p. 92.

clear that Wundt points directly toward a kinæsthetic explanation of rhythm.

While Meyer was investigating only speech rhythms, he seems to have stated positively that the consciousness of rhythm is not present until there is added to the perception of an objective series the feeling of bodily movement:

"Es sei ausdrücklich betont, dass, wo rhythmus und takt in frage kommen, es sich stets um bewegungsemfindungen handelt. Nicht der, der einem tanzenden zusieht oder einem ein lied rezitirenden zuhört, sondern der tanzende, der rezitirende selbst hat die unmittelbare emfindung des rhythmus. Gesichts- und gehörseindrücke vermögen nur mittelbar die emfindung des rhythmus zu erwecken, insofern diese eindrücke nämlich, von rhythmisch bewegten gegenständen hervorgerufen, bei dem emfindenden selbst rhythmische bewegungen des körpers auslösen. Es ist nicht nötig, das die bewegungen wirklich nach aussen hin sichtbar werden, obwohl dieses oft trotz des zwanges, den die sitte ausübt, eintritt."[1]

Smith recognizes the presence of a motor accompaniment of rhythm, which she describes as expressing itself after a definite temporal and intensive scheme, and not perfecting itself freely as in the progress of the usual emotion.[2] In relation to the question whether the effect of rhythm on work is to be regarded more as physiological or psychological, Smith has this further to say about rhythm and movement: "The origin of rhythm is in general physiological, *i. e.*, the regular repetition of bodily movements is the physiological stimulus, whose mental product is rhythm."[3] Again, she says: "The sensation of rhythm which one has from listening to singing, to dancing or to reading, is entirely different from the sensation of rhythm which one has when he himself sings, dances, reads, etc. It appears true that the foundation of rhythm lies closer to the functions of the general organs of the body than to the special sense organs."[4]

Squire observes the intimate connection between movements and the rhythmic impression. She notes that one subject could only give a unitary character to the group when he was making a spoken group correspond to a complete wave of respira-

[1] 'Beitr. zur deutschen Metrik,' p. 37.
[2] *Philos. Stud.*, XVI., 291.
[3] *Op. cit.*, p. 294.
[4] *Op. cit.*, p. 297.

tion. The trochee grouping in reciting a repeated syllable was coördinated with a complete wave of respiration. In other cases subjects depended on different movements. She finds that 48 subjects, for example, nodded their head while speaking dactyls. More important still is her observation that 'absence of movement was generally correlated with imperfect grouping.'[1]

The most recent description of rhythm by an experimenter is that of MacDougall. In one place he recognizes clearly that rhythm cannot be felt without there being present a kinæsthetic accompaniment. He says:

> "The bare auditory perception of a series of sounds, the uniformity of which is broken by periodic reinforcements, no more affords the peculiar experience of rhythm than does the perception of those visual symbols which represent the relations of such a series of sounds in musical notation. The successive stimulations must start a series of motor impulses somewhere before the rhythm is felt. Apart from such a pulse of bodily change the perception of a rhythmical series of sounds would be the bare abstract apprehension of their varying intensities and intervals."[2]

In another place he says:

> "We may conceive the succession of auditory stimulations to arouse a parallel motor accompaniment in the form of sensation reflexes occurring in some part of the bodily organism. The perception of rhythm under this conception is due to the kinæsthetic sensations whereby periodic elements of the primary auditory series are reinforced in such a way that the whole sum of sensational material rhythmically increases and decreases. * * * Here, also, there is present in consciousness a real series, but it is the accompaniment, not the original sequence of sensations, which is thus characterized."[3]

While MacDougall thus observes that rhythm is not felt without these kinæsthetic sensations, he goes further and conceives, as I have noted before, that the impression of rhythm is somehow also bound up with a constant nerve or brain pulse, *i. e.*, with a real change in the 'original sequence of sensations.' It is to this I would object. If he meant by a 'rhythmic functioning' of the nervous system the fact that there would be a rhythmic nerve current if we had rhythmic movement, of course I should agree. He seems, however, to hold that the rhythm of the nerve current is a constant organic rhythm continuing all the time. It comes in to reinforce certain of the

[1] *Op. cit.*, p. 79 ff.

[2] *Psychol. Rev.*, IX., 464. See also *Monog. Sup. Psychol. Rev.*, IV., 325.

[3] *Psychol. Rev.*, IX., 465.

original auditory elements. This I have objected to, together with other organic rhythms, on the ground that it would be a constant. That MacDougall includes both the kinæsthetic sensations arising in the presence of the outer stimuli and this constant nerve functioning in his explanation is shown in his summary which follows the statements quoted. He says: "Both of these relations between the rhythmically repeated stimulation and the nervous activity, namely, functional facilitation and reflex motor discharge, I conceive to be represented in the conditions which support the impression of rhythm."[1]

We see that MacDougall would thus explain rhythm in part by actual changes of the primary sensations and in part as illusion from the kinæsthetic accompaniment. In voluntary subjective rhythms we have little or no evidence of a real change in the intensity of the auditory sensations due to the period of nerve functioning. I believe it is better to hold, therefore, that the impression of rhythm arises only from the kinæsthetic sensations.

Stetson[2] offers some excellent 'suggestions for a motor theory of rhythm' in a paper which has just appeared. He outlines the application of such a theory to speech rhythms, but without committing himself to it. Assuming a motor explanation, he regards the feeling of 'tension between positive and negative muscle sets' as the conscious factor 'whereby all the beats of a period seem to belong to a single whole.'[3]

From the above references to various investigators it is plain that kinæsthetic sensations have from the first been observed in the background of the rhythmic impression. MacDougall approaches most nearly the formulation of a kinæsthetic theory, although others have hinted at it. I believe that it is time to recognize that it is this element in the conscious experience which adequately affords the explanation of the main fact of rhythm, the group feeling. To be sure we should not stop here. There are many other attributes of the rhythmic impres-

[1] *Ibid.*

[2] 'Rhythm and Rhyme,' *Harvard Psychological Studies*, I., 413–467; *Monog. Sup.*, *Psychol. Rev.*, IV., 413.

[3] *Op. cit.*, p. 455.

sion to be accounted for. There are questions of accent, of the genesis of rhythm, of the limitations of grouping, etc., to be investigated. The advantage of the kinæsthetic thesis which is offered here is that it gives a satisfactory mode of approach for these other problems. I do not expect to enter into the discussion of these problems at any length, but merely to point out the bearing which the kinæsthetic point of view would have on them. The rhythmic grouping, for example, must be limited by conditions which interrupt the kinæsthetic sensations, such as fatigue or the predominating influence of other sensations in consciousness. This furnishes a key to testing suggestions as to why grouping is lost below and above certain limits.

The question whether accent is necessary to the rhythmic impression has been a matter of some dispute. So far as the thesis which I am defending is concerned it is not a question of vital importance. Sensations of movement and tension may concur as well with an accented as with an unaccented group. They explain intensity changes in the units as well as temporal crowding of the units into groups. Meumann notes that with Hauptmann, Westphal, Lobe, Herbart, Schopenhauer and Lotze rhythm is conditioned by temporal connection of the units; while with Kostlin it depends upon accent.[1] Meumann himself, as well as Wundt and MacDougall, regards an accentual as well as a temporal connection to be necessary.[2] Squire is the most recent investigator to note the presence of rhythm without the perception of accent. Indeed, she regards the non-accented group as most early developed in the child, so far as its ability to speak rhythms is concerned.[3] My own subjects, both in observing sounds and lights, have often mentioned rhythmic groups set off by longer intervals without noting any difference in the units. It seems clear that it is not necessary to perceive accent in order to get the full grouping impression and affective tone of rhythm. Moreover, it is possible to conceive a movement sensation accompanying an auditory series and setting the sounds off into groups by like stages in

[1] *Philos. Stud.*, X., 250.

[2] Meumann, *Philos. Stud.*, X., 303. Wundt, 'Physiol. Psychol.,' 4te Aufl., II., 88 ff. MacDougall, *Psychol. Rev.*, IX., 460.

[3] *Op. cit.*, p. 50.

its wave without an illusion of accent. On the other hand a repeated increase in force of movement might easily be interpreted as an objective accent. I prefer, then, to hold that accent is not necessary to the rhythmic impression, although it is commonly present.

On the kinæsthetic basis the pleasure from rhythm is accounted for in rather suggestive ways. Squire, for example, has suggested that the pleasure arises first from the pleasure in mere activity. "The great pleasure which children find in rhythm is due to the efficacy of rhythm to set up vibrations in other organs of the body, and the consequent harmonious activity of the several bodily organs. The affective tone increases in proportion as the summation of excitation increases, till a state bordering on ecstacy may be reached."[1] MacDougall suggests that we take greater pleasure in certain rhythms because of the 'coincidence of subjective and objective change.'[2] We may say that we have a biological ground for pleasure in rhythm whenever we show that its tensions and movements favor the regular organic rhythms. The coincidence of both means a favorable condition for the individual and hence pleasure arising in a truly purposive way.

If we approach a genetic explanation of rhythm from the point of view I have outlined, we are ready both to describe the manner of origin and the use of this phenomenon in our mental life. Since it requires both objective stimuli and kinæsthetic sensations for its perfection, we recognize that the rhythmic experience arose only when the 'outer' sensations in series became accompanied by movement or tension. Under the general tendency of processes to repeat themselves[3] this developed into a regular perception of the sensations as grouped. The impression might first have arisen from a grouping by one of the prominent regular bodily rhythms like breathing and then been carried over into other movements. Bücher regards poetical and musical rhythms as developed from movements accompanying work.[4] The purpose of rhythm in mental evolution

[1] *Op. cit.*, p. 98.
[2] *Psychol. Rev.*, IX., 465.
[3] MacDougall, *Psychol. Rev.*, IX., 469.
[4] 'Arbeit und Rhythmus.'

might well be said to be the aid it affords in making automatic the perception of like events in series. A mental act which becomes automatic allows us to increase the range of contemporary activity. 'Economy of attention' or increased 'span of consciousness' are certainly results worthy of survival.

Psychological Summary.

In this discussion of rhythm from the psychological side it is apparent that physiological considerations have constantly come in. On account of the nature of the thesis offered it seems impossible to completely exclude them. This is especially true in considering the bearing of constant organic rhythms on the explanation. In summarizing this discussion, however, we can keep well within psychological terms. As the simplest description of rhythm we would say that it is the uniform recurrence of sensations of movement or tension concurring in regular periods with sensations from an objective series of stimuli. We reject the explanation of the main fact of rhythm — the unitary feeling of the group — on the basis of regular organic rhythms or on the basis of attention. We try to simplify Wundt's terms 'expectation and satisfaction' to tension and movement sensations. We refuse to classify rhythm as either a pure perception or pure emotion. In brief we say:

Rhythm is the uniform perception of successive groups of objectively localized sensations, accompanied by a characteristic emotional tone (the sensations of movement and tension coincident with the perception of the objective series). These kinæsthetic sensations provide the factor by which the unit sensations appear bound into groups and at the same time give the peculiar affective tone of rhythm. The perception is limited to sensations which follow not too rapidly to permit the kinæsthetic accompaniment to arise, nor too slowly to permit it being broken by other sensations. The most pleasurable rhythm is that in which the kinæsthetic sensations are reinforced by sensations accompanying the regular bodily rhythms, *i. e.*, when the two coincide. Genetically rhythm arose with the coördination of regular movement sensations and more rapid serial sensations. Biologically it was fostered because serving the purpose of economy.

B. From the Physiological Side.

1. *Demonstration of an Involuntary Movement Correlate of Rhythm.*

Turning more particularly to the physiological aspect of rhythm, I will first present my own experimental work with muscular responses to serial stimuli. The results furnish evidence of a physiological character bearing out the kinæsthetic explanation of rhythm which I have offered in the preceding pages. I began by investigating the effect of a series of like sounds on the muscular system, expecting to show that the stimulus after reaching the brain was, in some cases at least, diffused to the voluntary muscles. This would merely be an interesting demonstration of the generally accepted hypothesis of psychology, 'no impression without expression.' The research took on a more important character, however, when I found not only this diffusion, but also that the muscular responses did not agree with the incoming impressions. Instead, they combined the stimuli into temporally uniform group waves. Here I believed that I had found a rather complete correlate for the subjective perception of rhythm in a uniform objective series, and at the same time provided a graphic demonstration of that physiological condition.

Method of Experiment.

Before taking up my experiments in detail I desire to give some suggestions as to the difficulties to be overcome in getting satisfactory involuntary curves from the muscles. While the troubles encountered in my first work now seem less serious, it required many months of experimenting before they were obviated. Even at the present time it is by no means a simple matter to get an involuntary muscle curve showing grouping. For this reason I have reproduced a number of my curves in the plates as samples. The first and chief trouble which I met was the difficulty in getting a characteristic normal line when the subject's muscles were in such a state of unstable equilibrium that the sound stimulus would cause a movement. Breathing and the heart beat, as well as numerous accidental movements

of the body, head and limbs, showed themselves often in the normal line. Unless these can be eliminated by finding some muscular response where they are not also recorded, or which is plainly distinct from them, they will obscure the results. It is very desirable that the instruments should record a straight normal line when the stimulus is removed. This I succeeded in doing in the case of hand and forearm movements. Where the record was taken of head movements, breathing and accidental motions disturb the normal line, not sufficiently, however, to make it difficult to distinguish the stimulus line and its characteristics. I may say that I have made quite extensive tests with various parts of the body and have found a movement of the hand and forearm and movements of the head to be most satisfactory for this work. I have tried movements of the foot, of the leg, of the full arm, both with the subject standing and with him sitting; also movements of the tongue, all with comparatively little success. Somewhat to my surprise I could not succeed in registering a change in the squeeze of the hand, when a receiver was clasped in the grasp.

Another difficulty which my long experimenting, extending more than a year at interrupted intervals, convinces me will probably never be entirely overcome, is the impossibility of getting any involuntary movement curves from some subjects. In other words, I believe that not all subjects can relax themselves sufficiently to stop inhibiting their involuntary tendencies to move. Altogether I have tested at length fourteen subjects. From five of these I could get no results whatever. The reason for this I will discuss further on.

The sound stimulus which was used in these experiments was given by a metronome placed on the table beside the subject. The bell attachment was used when I wished to give an accented rhythm. The ordinary beats of the metronome were sufficiently uniform for the subjects to subjectively group them in twos or threes. The fact that the subjects easily accepted the illusion that every third beat was accented shows that the sounds were uniform enough for my purpose.

For recording the movements of the subject I have used two different methods. In the greater part of the work I used a

receiver consisting of a light sponge, about half the size of the closed fist, inclosed in a thin rubber air-tight bag.[1] This was connected to a Marey tambour. I tried numerous forms of receivers, including the bell-shaped pneumograph and various rubber bulbs; but the sponge was the most satisfactory, as it could be adjusted to the hand in any position so as to fit snugly. The other method, with which I took a few of the later head records, consisted in passing a thread directly from an object held in the mouth of the subject to the recording pen. The thread was held taut by a thread passing in the opposite direction from the recording lever over a pulley and pulled by a slight weight. This could be easily accomplished, as the pen was adjusted to write on a horizontal kymograph, the subject sitting opposite one end of the drum.

The first records were made on a kymograph using smoked paper, but at Columbia University I substituted an ink record on a continuous-roll kymograph. This was possible by using a glass pen which fed by capillary attraction from a small inkwell attached to the tambour. This form of pen has been used in psychological research by Wissler, Bonser[2] and others. It may be drawn out from a small glass tubing and is so convenient that it deserves wider adoption. The pen may be attached to electric markers or used with the tambour for recording blood pressure, pulse and breathing.

When experimenting on a subject I had him sit beside a table on which one hand rested. The outside edge of the hand was down and acted as a sort of fulcrum on which the hand might sway sideways with ease. The subject's elbow rested on the arm of his chair so that the position was comfortable. This position was chosen after much experimenting, as it was the only one found in which no movement was recorded except when the metronome was beating, and which was yet sensitive enough to get a result with certain subjects under the sound stimulus. The bending of the elbow and resting of the hand seemed to obviate respiration and other movements. The sub-

[1] Suggested by Dearborn and Spindler, 'Involuntary Motor Reactions to Pleasant and Unpleasant Stimuli,' *Psychol. Rev.*, IV., 453.

[2] 'A Study of the Relations between Mental Activity and the Circulation of the Blood,' *Psychol. Rev.*, 1903, X., 120.

ject was told to relax all his muscles as much as possible and then listen to the metronome, with eyes closed. After he had listened awhile to the beats I asked him if the sounds seemed to vary in any regular way. He generally at once noticed the grouping. Having made sure that the subject perceived a subjective rhythm I then watched for involuntary movements. If found at all they usually appeared in the hand or head. The subject was sometimes wholly unconscious of the movements. In any case it was necessary to be sure that the movements were not voluntarily made. The question whether the movements were involuntary was not troublesome on account of the trustworthiness of the subjects chosen. It could be judged by the attitude of the subject as well as his statements. The subject was always cautioned not to make any voluntary movements.

The Individual Records.

Subject *A*. A senior, male, at Minnesota University. He was able to relax himself exceptionally well and I obtained a very complete series of curves of the hand and forearm movement by the method described above. The effect was recorded when the metronome was beating without the bell attachment at the speeds 40, 66, 120 and 200 per minute. At the speed 40 the record (Fig. 1) shows one muscle wave for each beat. This is changed to one wave for two beats at the speed 66 and one wave for four beats at the speed 200. In another record with the same subject I found the grouping by fours at the speed 120. I also found that at another time he grouped the 40 speed in twos. Again, we find that when the bell of the metronome was added to every alternate beat at the speed 66 the record shows a single wave to each stimulus instead of one wave covering two stimuli as before. The same result followed this speed when the bell was added to every third and to every fourth beat, with the difference that when the bell was on every fourth beat the wave corresponding to the accented beat is noticeably higher than for the others. The subject was certain that the movements were not voluntary. To quote from him: "I was conscious that my hand was not still, but usually did not realize that it was making any regular movement. The motions were at no time voluntarily made." The early records with this subject were obtained without my suggesting in any way to the subject that he think of the sounds as grouped. He was told merely to listen to the sounds and relax himself. It was after the records that I found that the sounds had appeared in rhythms to the subject.

Subject *B*. Graduate student, male, at Columbia, a trained introspector. No regular effect was noticed for his hand, but the head movement was pronounced and I recorded that by the thread passing to the writing lever in the manner described above. The subject could easily think of the sounds as grouped in twos or threes, in which case the first sound of the groups appeared to be accented. Curves of this subject are shown (Fig. 2) when he was group-

ing by twos with the speed of the metronome 120 per minute, and for a three-group when the speed was 72; also for an objective two-rhythm, the bell producing the accent, at the speed 120. All of these curves show one wave to the group. An interesting variation is shown in curve *C*, an objective two-rhythm

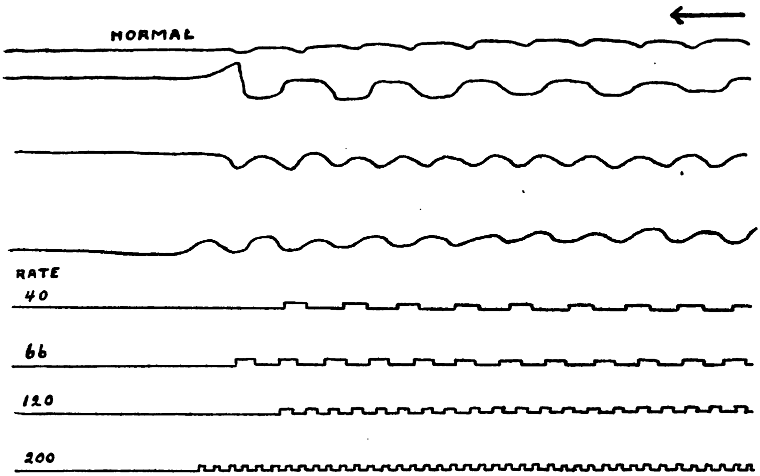

FIG. 1. Involuntary rhythmic movement of the hand. Subject *A*. Curves read from right to left. Reduced to one half original size.[1]

with the speed 120, *i. e.*, the bell sounding on every alternate beat. In this case the general wave of the group shows a higher rise for the accented sound followed by a lower rise for the unaccented, but both combined in a group swell. *B*'s introspection was as follows: "I can easily group the sounds of the metro-

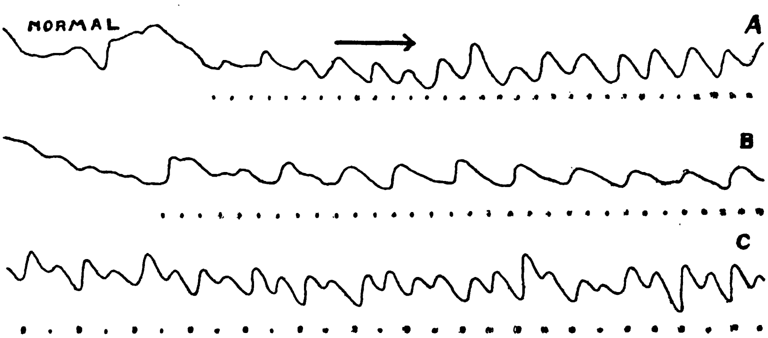

FIG. 2. Involuntary rhythmic movement of head. Subject *B*. Curve *C* shows the typical effect of an objectively accented rhythm. The dots are drawn in schematically to show the position of the stimuli. Curves read from left to right. Reduced to one third original size.

[1] The muscle curves shown in this and the following figures were accurately traced off from the originals. This was found necessary in order to print them in a satisfactory manner.

nome in either a two- or three-rhythm. The first sound in the group seems to be a little louder. When I relax myself and do not inhibit the tendency to movement which each sound seems to start in me, I am conscious that I am moving; but this movement is in no sense voluntary. Several times I was quite unconscious that it was regular at all, or that there was a movement for each group of sounds." After curve *A* was traced subject *B* stated that he was entirely unconscious of any uniform movement of his head.

Subject *C*, female, junior at Minnesota University. Curves were obtained for her hand movement, similar to those of subject *A*, but not so pronounced. A curious individual peculiarity in her case was the fact that, even when the metronome was beating at its slow speed of 40 per minute, she did not show a separate wave to each stimulus. Two beats were always included in each muscle wave up to the speed 112, when she changed to a grouping of four beats to the wave. The grouping movements were noticed without any suggestion on the part of the experimenter and before the receiver was adjusted in place. The subject, when cautioned against voluntary movements, stated positively that her hand was moving entirely without her control.

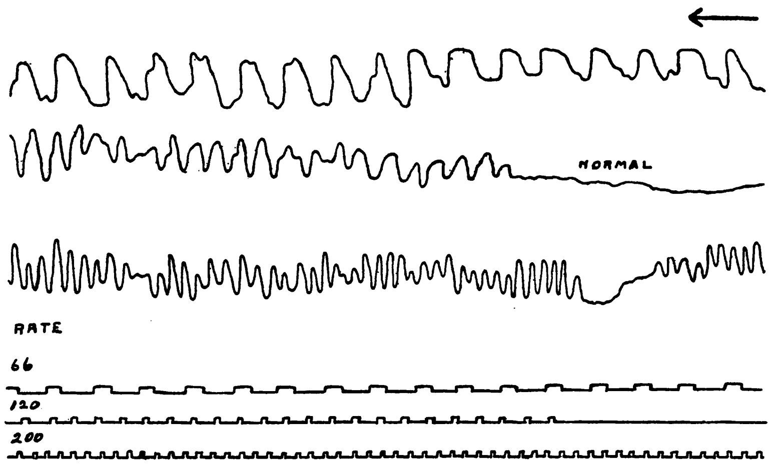

FIG. 3. Involuntary head movement, one wave to each stimulus even at the rapid rates. Subject *D*. Curves read from right to left. Reduced to one half original size.

Subject *D*, male, a senior at Minnesota. He showed no movement that I could detect with his hand, but had quite a pronounced head rhythm under stimulation. On account of its character it could not, I believe, be duplicated voluntarily. He was to outward appearances more relaxed than any of the other subjects I worked with. To this fact may be attributed the peculiar effect obtained. Even at the highest speed of the metronome, 200 per minute, the record shows a separate muscle wave for each stimulus given. See curves in Fig. 3. If one tries voluntarily to keep up a nodding of the head at the rate of three per second, he finds it practically impossible. It certainly could not be accomplished without great effort and in this case was entirely without effort, virtually reflex in character. According to the subject's statement it was absolutely involuntary.

Subject *E*, instructor in psychology at Minnesota. No hand response detected. A record of his head movement was recorded using the sponge receiver. The record was satisfactory for the speed of 120 per minute, at which the subject showed one muscle wave for each two beats. The tests were not continued for other rates of speed.

Subject *F*, senior, male, at Minnesota University. No movement of the head or hand detected, but the subject was found on observation to be making a regular rhythmical movement of his eyelids. When questioned as to the movement the subject said: "I did not know that my eyelids were twitching or moving in response to the sounds. I was wholly unconscious of the movement." Two other observers in the laboratory confirmed the rhythmical movement of the subject's eyelids, but the movement was not registered on account of the special apparatus necessary.

Subject *G*, assistant to the instrument maker at the Columbia laboratory, gave no results with the hand and only rather uncertain results with the head. His normal line was so disturbed that it obscured in most cases the effect of the sound stimulus. A grouping appeared at times, but so irregularly that little definite can be said about it. The thread recording method was used.

Subject *H*, graduate student, male, at Columbia. One good curve was obtained with this subject. It was a record of grouping with the head movement when the bell was sounding on every alternate beat at the speed 184. The grouping includes two bell stimuli for each wave. The bell probably caused the movement where the beat of the metronome failed. This was the only satisfactory curve obtained after many trials with this subject. The record was made directly by the thread attached to the tambour lever.

Inhibited Responses.

A series of curves which I obtained from one subject has a direct bearing on the question why we do not obtain involuntary muscle waves from all. Normally this subject, *J*, gave no response whatever to the metronome beat with her hand, head or body. She tried to relax her muscles completely, but still there was no movement. The remarks of my subjects and my own introspection seemed to me to explain adequately this non-responsiveness of certain subjects. Every person in his waking state tends to inhibit all random and involuntary movements. We normally hold our muscles in some degree of tension. I am confident that this is true of myself, even when I try to let myself relax as much as possible. If it were not so, every stimulus affecting us would doubtless cause useless movements which would seriously disturb us in our work. Dearborn and Spindler in studying involuntary motor reactions to pleasant and unpleasant stimuli noted that 'some subjects seemed constitutionally averse to any motor reaction.'[1] In the second

[1] *Psychol. Rev.*, IV., 454.

place it was apparent when I observed the subjects, that some of them could relax themselves much more than others. Relaxation seemed to be accompanied by a let up in the 'inhibition process' by which our voluntary muscles are held quiet unless we will to move them. I concluded that, if this usual waking inhibition could be eliminated, I might be able to get results from a subject, where normally there was no such response discoverable. Hypnosis offered a rather promising method of completely relaxing a subject. Subject *J*, mentioned above, who was an instructor at Minnesota, and a trained introspector, was found to respond readily to the suggestion of sleep. Hypnosis was induced by looking at a bright object in the usual manner. When *J* was sound asleep she showed the usual hypnotic condition by accepting simple commands like, 'you cannot bend your arm,' etc. I then started the metronome and merely told her to listen to it. Her hand, resting on the table, now seemed to be giving spasmodic jerks. These were, however, quite irregular. I placed the receiver against her hand and directed all my attention to that, so far as the subject could tell, for she had her eyes closed. In the normal waking state this subject had only been tested for hand movements, although I watched for others and found none present. I had never suggested nor discussed with this subject the possibility of any form of response other than the hand. I dwell on this to emphasize the fact that so far as any suggestion had been given the subject it was for an expected movement of the hand. This seems to me important evidence that the results afterward obtained were not the result of suggestion. While still trying to adjust the receiver to record the uncertain hand movements, I noticed that the subject's head was moving decidedly. On studying these movements carefully I noticed that they showed the muscle-wave grouping exactly like that I had obtained from other subjects in the normal state. After I was sure of these observations, I adjusted the receiver to the subject's head and made the tracing *a*, Fig. 4. The first curve was made with the metronome beating 66 per minute, without the bell attachment. The only direction given the subject was to listen to the sounds. The wave shows the charac-

teristic grouping by threes. The speed was then changed to 120 and the subject was told to listen and not move her head. The group wave disappeared at once. Then the subject was told she need not inhibit the movement of her head. The result was at once a muscle wave for every four beats. When told not to move her head the muscle wave again became straight. See curve *b*, Fig. 4. The direction to inhibit movements at once produced the straight line like that of the subject under stimulus in the normal waking state. After stopping to replenish the smoked paper on the kymograph, I set the metronome at 120 and said nothing to the subject. All this time it should

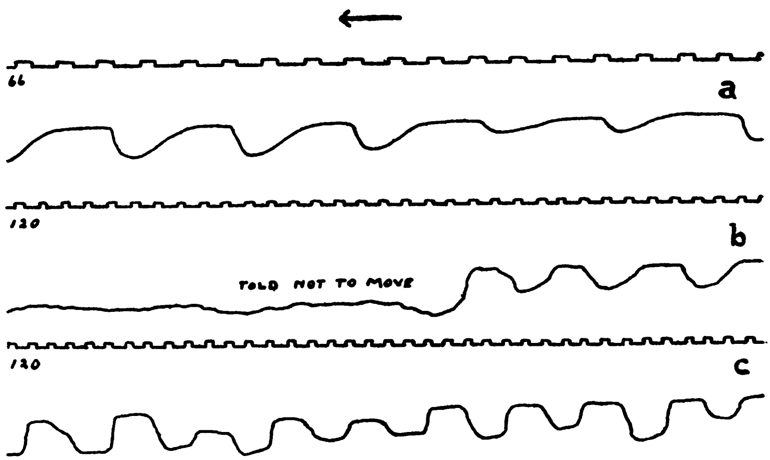

FIG. 4. Rhythmic head movement under hypnosis. Subject *J*. Curves read from right to left. Reduced to one half original size.

be remembered that the subject's eyes were closed, so that she had seen none of the tracings on the drum. Without any direction the subject again took up the grouping in fours. I next tried to determine whether the subject could be made by suggestion to group these beats in twos instead of fours. I told her to respond to every two beats with her head and I counted the beats '1, 2, 1, 2, 1, 2,' etc. The suggestion failed to make her abandon the four-grouping, although the curve (*c*, Fig. 4) shows an interesting tendency to give half of the complete wave to each two beats, thus subdividing the group. Changing the speed to 66 per minute I tried to get the subject to respond with

a head movement for each stimulus. This again was impossible, although I counted '1, 1, 1, 1,' with the stimuli. She continued to make one movement cover two beats as she had done before without suggestion. Finally I suggested that she group these beats in fours and the suggestion was immediately responded to by a movement wave extending over four beats. The same thing occurred with a three-grouping. I stopped counting the beats after a few groups and the subject continued to preserve the wave movement to three beats.

Summary of the Muscle Curves.

The empirical results which we may gather from these curves may be summarized as follows: (1) The diffusion of the metronome sound stimulus to the voluntary muscles, under some circumstances, is demonstrated. (2) Without voluntary direction these movement responses under subjective rhythmic conditions are something more than one response to each stimulus. They take the form of one muscle wave covering a group of stimuli. (3) These rhythmic muscle waves vary at different times in the same individual as to the number of unit stimuli which they embrace (see Subject *A*). They also vary with different subjects. Compare Subjects *A* and *C* as to the grouping at the speed forty per minute. (4) The form of the muscle wave is a smooth curve, or a curve which starts rather abruptly and sinks back more slowly. Under the latter form of curve, the longest part of the movement was between the last metronome beat of one group and the first of the next. This was the most common condition. (5) Muscular relaxation on the part of the subject seems to be a necessary condition for getting satisfactory results. Holding the muscles tense, as is done normally in the waking state, inhibits the movements. It must be overcome by sufficient relaxation or by hypnosis. When the relaxation was brought about by hypnosis, muscle waves were obtained where no response had previously been noted. See Subject *J*. This response was apparently without suggestion.

2. *Revised Physiological Explanation.*

When we undertake the explanation of any state of consciousness from a physiological point of view our purpose is to find

the organic conditions which correlate with that particular state. The connection of these bodily conditions and the external stimulus in a chain of cause and effect would then complete the explanation. The most characteristic feature of the experience of rhythm which demands explanation is the fact that stimuli, occurring regularly in series, appear in consciousness to be united into groups. In the psychological discussion we have found that kinæsthetic sensations would provide an adequate element for the group feeling, provided that it could be demonstrated that they were actually present. My experiments show movements to be present in certain cases in a form which correlates with what we have termed a group feeling. I have already referred to other investigators, notably Bolton, MacDougall, Meyer and Squire, who have observed motor accompaniments during rhythmization. A further quotation from Bolton states the general observation in a way with which nearly all observers would agree. He says:

"Most subjects felt themselves impelled by an irresistible force to make muscular movements of some sort accompanying the rhythms. If they attempted to restrain these movements in one muscle, they were very likely to appear somewhere else. * * * The most common forms of muscular movement were beating time with the foot, nodding the head or swaying the body. Subjects 3, 10 and 17 accompanied the rhythmical grouping by muscular contraction of the diaphragm and chest, and it was exceedingly difficult to restrain them. Other subjects counted inaudibly or made the proper muscular adjustments for counting. Slight or nascent muscular contractions were felt in the root of the tongue or larynx. Most subjects were unconscious of their muscular movements until their attention was called to them and subject 15 never became conscious of the rhythmical contractions in the eyelids."[1]

It may be a long step from explaining rhythm on the basis of movements which are sometimes present to explaining it always on a kinæsthetic basis. This apparent jump does not seem so great, however, in view of several considerations brought out in connection with my experiments. In the first place the muscle wave correlates with several attributes of the rhythmical impression which other bodily conditions that have been suggested do not. It varies, for example, in length of its period with the rhythm perceived. As the unit sensations come more rapidly the muscle wave includes more units just as the

[1] *Amer. J. of Psychol.*, VI., 234.

conscious group does. It varies at different times in its form of grouping for the same rate of stimuli. This variation was noticeable for the same individual and for different individuals. The wave appeared in both a purely subjective rhythmization and when an objectively accented rhythm was given. All these characteristics of the muscle wave point to its being a true correlate. Moreover, my experiments with inhibited responses indicated that because movement is not always observed probably does not mean that there is no muscular activity in such cases. As soon as the normal waking inhibition was removed we had movement manifested. Meyer has stated that the activity need not be visible in order to give the feeling of movement.[1] MacDougall makes an important observation in this connection: "Of greater prevalence but much more difficult of observation are contractions giving rise to sensations of strain in the throat, head, chest and limbs, tensions in the vocal and respiratory muscles, and above all the simultaneous innervation of opposed sets of extensor and flexor muscles producing alternate phases of rigidity and relaxation which do not affect the local relations of the organ in which they take place."[2]

Thus we may suppose that the inhibition of a movement would substitute a new condition of activity in the muscle, a condition of 'rigidity and relaxation.' These conditions would arouse kinæsthetic sensations as much as would actual movements, but here the sensations would be varying degrees of tension. I therefore include both tension and movement in describing the conscious element necessary to account for the group feeling. Mach has called attention to the fact that an attempted movement of the eyeball, when movement is actually blocked by a piece of putty in the socket, gives an effect similar to a movement performed.[3] Practically all subjects on careful introspection note at least a feeling of 'tendency to move,' even when they do not speak of strain or movement sensations. On better analysis this feeling of 'tendency' seems not different from a real tension sensation in some of the muscles of the body. There seems to be nothing strange in the fact that these kinæs-

[1] *Op. cit.*, p. 37.

[2] *Psychol. Rev.*, IX., 466.

[3] 'Analysis of Sensations,' trans. by Williams, p. 59.

thetic sensations are interpreted as changes in the external series: that objectively uniform sounds appear accented and grouped. We are familiar with the usual explanation of the vertical line illusion, that eye movements along vertical lines, because accompanied by more effort, make the lines appear longer. Considering all the facts, there seems to be as good reason in the case of rhythm to suppose that muscular activity (movement or tension) is at the basis of the impression.

This explanation of rhythm is to be supplemented by the supposition that memory of past movements may give the feeling of rhythm without actual repetition of the muscular activity, A man whose arm has been removed may at times still feel as if it moved. The effect is here explained as the excitation of brain centers by some associated tract. So we may suppose with rhythm that the kinæsthetic group feeling, formerly present with an actual muscular activity, is at a subsequent time merely revived in memory by association with the auditory sensations. Memory does not require muscular activity to be repeated. Rhythm would then be due solely to the memory of past activity. In the great majority of cases, however, the feeling of movement or tendency to move connected with the rhythmic impression is so distinct that it is difficult to suppose it can be a mere memory. In these cases a tension of the muscles seems very likely when movements themselves are not observed.

Analogies to the Group Wave.

Having gone so far as to suggest that the impression of rhythm depends upon a muscle wave or tension covering a group, we are called upon to show how it is that, if the stimuli come regularly, we can get a grouping movement. It is well to pass by the explanation of this rhythmical movement following an objective change in the series of stimuli or following voluntary grouping, in which cases the changing movement might be explained through factors connected with the stimulus itself or with volition. In the most important psychological aspect of rhythm, viz., involuntary subjective grouping, it seems to me we can suggest the direction in which we may hope to find the explanation, even if we cannot actually dis-

cover it. From the nature of the phenomenon it must be some structural arrangement of our body by which a series of like impressions, diffusing to the muscles, produces not a separate wave for each impulse, but a longer wave covering a group of impulses. Do we have any analogy for such a process? A number suggest themselves. In the first place the accompanying curve, Fig. 5, shows that in the case of electric stimuli applied to the thumb at intervals of .3 sec., one of the stimuli being slightly stronger than the succeeding two, the two like stimuli produced one wave with a break in its crest. This is just what we would expect under our hypothesis of grouping. In this case it appears to have been purely a reflex nerve and muscle action. Richet shows curves of muscular contraction when stimulating the cortex of a dog regularly with like

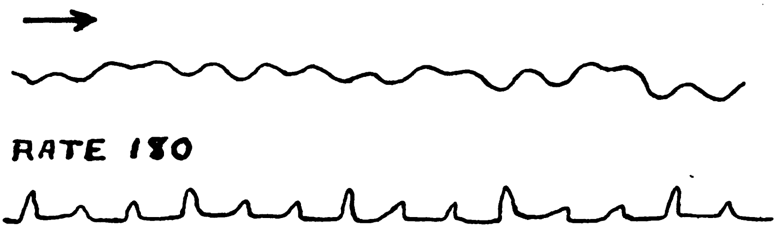

FIG. 5. Reflex grouping by the muscles of the thumb. Electric stimuli applied at the base of the thumb, every third slightly stronger. Subject *B*. Curve reads from left to right. Double the original size.

stimuli in which each alternate contraction is exaggerated, and finally in which there is only one contraction for every two stimuli.[1] The fatigue curves of Lombard show an analogous combination of muscular movements into groups in spite of attempted like voluntary contractions. He found that, after the second finger had fatigued while pulling every 2 sec. on the Mosso ergograph, it continued to show a rise and fall in the strength of its pull. A series of the same voluntary impulses thus produced a wave result. In this case, the succeeding waves are not of the same period, but they still show a grouping phenomenon under apparently like central stimuli. Lombard's wave result in the case of a continuous stimulus applied to a frog is of similar import. He says of this:

[1] *Revue Philosophique*, XLV., 341.

"In experiments made by the writer in Leipzig, 1884,[1] he found that if a constant temperature, high enough to cause reflex movements, but not so high as to rapidly destroy the tissues, were constantly applied to the skin of the leg of a decapitated frog, the resulting reflex action was not a continuous tetanus, but a series of tetani, which followed one another with considerable regularity. In the intervals between the tetani the muscles entirely relaxed, and the succeeding tetani were of nearly the same height. This phenomenon seemed to be dependent upon almost rhythmical changes occurring within the spinal cord."[2]

Bolton[3] refers to a number of similar analogies of grouped action under like stimuli. He cites Sterling to the effect that "submaximal stimuli following one another, even as slow as one per second, will produce a contraction after a time.[4] * * * Dr. Ward determined that between the rates of .4 and .03 sec. a contraction always followed a given number of stimuli. Above and below these limits the number might vary.[5] In the same line is the work of Drs. Kronacker and Hall.[6] It has been held by Sterling and others that when a stimulus is applied directly to the cortex, no matter what the rate, the brain sent out rhythmic impulses always at a constant rate."

Unless it should be said that after rejecting regular organic rhythms I am myself resorting to them for an explanation of rhythm, let me emphasize that I am here only speaking of analogies to the structural arrangement which I conceive must exist if repeated like stimuli are to produce a grouped movement. Muscular movements need not, however, be like the rhythmic brain pulse last mentioned in always being of the same period in spite of difference in the rate of stimulation. Because respiration, pulse, retinal rivalry, etc., show a constant period does not argue that the voluntary muscles may not have different periods of activity according as the stimuli come more or less rapidly. This period may vary again with the set of muscles in action. The above analogies indicate that the mus-

[1] *Du Bois-Reymond's Archiv*, 1885.

[2] *Amer. J. of Psychol.*, III., 41.

[3] *Amer. J. of Psychol.*, VI., 153.

[4] 'Ueber die Summation electrischer Hautreize.' *Leipzig, Berichte d. Sächs. Gesellschaft d. Wissenschaften*, 1874, p. 372.

[5] 'Ueber die Auslösung von Reflexbewegungen durch einer Summe schwacher Reize.' *Archiv für Anatomie und Physiologie*, 1880, p. 372.

[6] 'Die willkürliche Muskelaction.' *Archiv für Anatomie und Physiologie*, 1879.

cles may manifest periodic activity in the presence of serial stimuli. The muscle curves obtained by me indicate, as the kinæsthetic theory of rhythm demands, that the period of the muscle wave varies with each change in the outer stimulus and with the general condition of the subject.

No analogy, of course, proves that the grouping of like impulses in a waved muscular activity is what occurs in the case of listening to a series of like sounds. Analogies are important, however, as indicating that similar processes take place within us. They show that the present hypothesis in the case of the perception of rhythm would not introduce an entirely different physiological process. Moreover, I believe that my experiments supplement these analogies in the definite perception of rhythm and are in line with the direction from which most may be expected in connecting this experience with a physiological chain of cause and effect.

Limitations of Grouping.

It has been an ambition with nearly every investigator of rhythm to show that the limitations of rhythmic grouping and the favored length of group were dependent upon some particular bodily rhythm. For this purpose waves of respiration, circulation, attention, fatigue, the regular period of walking and various other physiological serial repetitions have been brought into the discussion. The result is far from satisfactory. The variations are so great and the evidence from different sources so conflicting that it is safe to say no definite connection has been traced, except possibly for the highest rate of stimuli at which grouping occurs. There are apparently three factors in the experience for which some source of limitation must be sought. (1) The fact that stimuli coming too rapidly cannot be grouped. (2) That the length of the group does not increase proportionally to the number of elements in it. How closely it tends to remain the same length is still uncertain. (3) That grouping ceases when the units are too far apart.

1. Investigators apparently agree that sounds must not occur faster than .1 sec. in order voluntarily to be grouped.[1] This

[1]Bolton, *Amer. J. of Psychol.*, VI., 237. Wundt, 'Human and Animal Psychol.,' Titchener and Creighton trans., 263. Squire, *op. cit.*, 85.

approximately coincides, as Bolton pointed out, with the average number of voluntary muscular contractions possible in a second, or, as Schaefer puts it, the 'consecutive nervous discharge from the cortical cells.'[1] Richet showed that the fastest rate at which separate like contractions were produced by stimulating the cortex of a dog was with stimuli .1 sec. apart. Faster than this there was a summation of stimuli.[2] He also calls attention to the fact that ten or twelve syllables are all that can be articulated in a second.[3] Another interesting suggestion has been made that .1 sec. agrees with the shortest time in which it is possible to attain a complete simple perception, *e. g.*, to distinguish a color from a group, as calculated by Cattell.[4] We seem to reach at this rate a fairly constant average as to the limit of central processes. We have here, then, not only a limit for rhythm but for any simple mental process dealing with successive activities.

2. The connection of a favored length of group with any particular bodily rhythm is apparently now in a state of almost pure conjecture. In the first place we know that the same individual varies greatly in the length of the group he chooses. To illustrate, Ebhardt found with free tapping that one subject varied at different times in the average speed of tapping a two-rhythm from a group length of .625 to 1.148 sec. While a three-rhythm was tapped by the same subject at different times with average group lengths from .940 to 1.803 sec.[5] Not only does the individual vary for the same group, but the two-group is considerably different from the three-group, as shown by Ebhardt and by the experiments of Miyake in tapping on a noiseless key and on a drum; also by Squire for repeating a syllable in trochee or dactyl rhythms. In Miyake's tables an average of the two-group with the first unit accented while tapping on a noiseless key shows a length of group about 1.2 sec. and for a three-group, 1.9 sec.[6] With Squire the length of

[1] 'Text-Book of Physiol.,' 1900, p. 798.
[2] *Revue Philosophique*, XLV., 342.
[3] *Ibid.*, 348.
[4] *Mind*, XI., 383.
[5] *Ztsch. f. Psychol.*, XVIII., 116, 118.
[6] *Studies from the Yale Psychol. Lab.*, X., 15.

the two-group in the different school grades varied in its average from 1.3 to 2 sec., and of the three-group from 1.8 to 3 sec.[1] Each of these figures is my average of their tables of averages from numerous trials. Of a different nature are the times for the preferred length of group when listening to sounds. Bolton gives a table showing the average for his subjects to be 1.59 sec. for the two-group, lowering to 1.16 sec. for an eight-group.[2] MacDougall finds that his subjects, while listening to sounds that varied in rate from .08 to 1 sec., chose groupings which varied in length of the highest group chosen from .917 to 2.5 sec.[3] Of course the results given above are not strictly comparable. The time of a group might be very different for free tapping than for subjective grouping when listening to sounds, or might vary with the kind of sound, etc. The important fact to be noted is that the group, so far as the experiments yet show, cannot be regarded as a period which may be said to have a favored length, even approximately. Averages under different conditions, when the subject chooses his own period, vary at least between .9 and 3 sec.

Looking at the matter from the other side, there is almost as much disagreement as to the length of the organic rhythms chosen as the correlate for the favored rhythmic period. Certainly there is variety enough, so that almost any group period might find a correlate somewhere within the limits. The 'wave of attention,' for example, was fixed by Lange for light stimuli on an average at 3.4 sec.;[4] by Münsterberg at 6.9 sec.;[5] by Lehmann at 12.8 sec.[6] For sound and electric stimuli it is still different.

Lombard found no regularity in the period of fatigue and reinforcement of a muscle.[7] In 90 per cent. of 2,000 cases the normal respiration wave was from 2.5 to 4 sec. The average pulse rate for adult males is 72 and for females 80 per minute.

[1] *Op. cit.*, table 42.
[2] *Amer. J. of Psychol.*, VI., 214.
[3] *Monog. Sup. Psychol. Rev.*, IV., 329.
[4] *Philos. Stud.*, IV., 404.
[5] 'Die Schwankungen der Aufmerksamkeit,' Beiträge II., '69.
[6] *Philos. Stud.*, IX., 79.
[7] *Amer. J. of Psychol.*, III., 29.

The rate of the full double swing in walking has been placed at .664 sec. on the average.[1] Stevens found the vaso-motor wave varied in length from 5 to 20 sec. while his subjects were making time judgments.[2]

Bringing these facts of the period of grouping and organic rhythms together shows at once how misleading it is with our present knowledge to claim that there is a standard length of group or that the normal group depends upon respiration, fatigue or any particular physiological rhythm as determining its natural length. Before this question can be settled we must find out if there is a favored length tending to hold under varying conditions. If an average period can be found with a comparatively small variability, we shall be ready to see if it lies within the limits of any single bodily rhythm.

3. As to the time which the sensations may be apart and yet be grouped, there is considerable difference of opinion. Bolton fixed it at 1 sec. but he was thinking particularly of involuntary rhythm.[3] Wundt sets it at 4 sec.[4] and MacDougall at 1.5 to 2 sec.[5] I would suggest that this limit is probably set by some bodily rhythm which comes in so forcibly as to interrupt the impression of rhythm coming from a kinæsthetic source. A change in respiration might cause such an interruption, or the limit may be a fatigue for this particular state of tension or movement. Until there is some agreement as to the limit it is rather fruitless to conjecture further about its physiological accompaniment.

Effect of a Motor Explanation.

In concluding the part of this paper devoted to the physiological side of rhythm I wish to call attention to some of the advantages and effects of the revised explanation which I have offered. It shows the uselessness of trying to demonstrate that subjective rhythmic groups of widely varying length are corre-

[1] For these rhythms see Schaefer, 'Text-book of Physiol.,' I., 747; II., 102, 269.

[2] *Amer. J. of Psychol.*, XIII., 26.

[3] *Amer. J. of Psychol.*, VI., 237.

[4] 'Human and Animal Psychol.,' Titchener and Creighton trans., p. 263.

[5] *Monog. Sup. Psychol. Rev.*, IV., 322.

lated with any regular organic rhythm which has an approximately uniform period of its own, like the pulse or breathing. It gives a place for bodily rhythms in explaining the tendency to favor groups of a certain length and for other limitations in grouping. In this connection it suggests that greater pleasure arises from objective rhythms which most nearly coincide with our normal organic periods. The motor explanation of rhythm holds as well for rhythms which are given objectively as those in which no impulse for grouping is directly given by the external stimuli. As for subjective rhythms it correlates both for involuntary and voluntary grouping. In considering the form of the rhythmic group this explanation on the basis of muscular response is suggestive, not only as to the number of units within the group, but also as providing a simple account of the appearance of accent through stronger muscular action. We have already noted that the muscle wave theory is helpful in explaining the origin and development of rhythm. Finally, I wish to emphasize particularly that the motor explanation of rhythm destroys, in my opinion, all reason for supposing that the experience of rhythm is limited to two sense orders, hearing and movement, as has been often assumed. There would be no reason *a priori* why a series of stimuli addressed to any sense should not produce an experience of rhythm. I am quite confident that they would under proper circumstances; that rhythms of smell, taste, touch or vision are just as possible as rhythms of hearing. It will be one of my tasks in the next part of this paper to give some introspective evidence showing the presence of the rhythmic impression from serial light flashes. This visual rhythm, in my opinion, is identical in all its essentials with rhythms of sound.

PART II. VISUAL RHYTHMS.

1. Is Rhythm Limited to Hearing and Movement?

This question is raised by the general supposition among writers on rhythm that this particular experience is confined to certain orders of sense. Külpe says: 'It is a curious fact that the involuntary rhythmical apprehension of stimuli is confined to auditory impressions.'[1] Titchener leaves out the qualification 'involuntary' and says positively: "There are only two classes of sensation that can form the basis of the perception of rhythm. These are the auditory and the tactual or 'motor'; there can no more be, *e. g.*, a visual rhythm than there can be an auditory symmetry."[2] Squire says that rhythm 'appears to be a phenomenon characteristic of but two modalities, audition and movement.'[3] Meumann says we do not know why rhythm is limited to definite sense fields.[4] He apparently inclines to extend the experience in a lessened degree to other sensations than sound and movement. He suggests that: 'Jedes Sinnesgebiet um so mehr an den rhythmischen Erscheinungen Theil nimmt, je mehr und je exclusiver es Organ der Zeitschätzung ist.'[5] Hearing, being the best sense for time, is most adapted for receiving rhythmic impressions. Movement, serving also for space valuation, is less adapted; while sight, which is a dull sense for time and almost exclusively a space sense, he thinks is still less serviceable for rhythm. The only suggestion from the physiological side why we might consider rhythm limited in its field, is Mach's early conception that we have a special sense for rhythm which is localized in the organ of hearing.[6] Ewald describes a tonus organ for the muscles con-

[1] 'Outlines of Psychol.,' Titchener's trans., p. 389.

[2] 'Experimental Psychol., Qualitative, Student's Manual,' p. 174.

[3] *Op. cit.*, p. 85.

[4] *Philos. Stud.*, X., 249.

[5] *Op. cit.*, p. 261.

[6] 'Untersuchungen über den Zeitsinn des Ohres,' *Wien. Sitz.-Ber.*, 1865, II., 133.

nected with the labyrinth of the ear, which suggests a possibility of more intimate relations between hearing and movement than between other senses and the muscles.[1] Mach, however, gave up his theory of the ear organ for rhythm and referred time and rhythm both to a special energy of the brain.[2] Physiological suggestions, so far as they go, may then be regarded as reasons for hearing being a favored sense in rhythm rather than an exclusive sense.

My experimental work in visual rhythm has taken the nature of an introspective study. It has been directly concerned with this question of the limitation of rhythm to two sense orders. I wished to determine whether a series of identical light flashes might not also give the same experience of rhythm. I shall contend that the introspections of 26 subjects, with whom I have worked, show that we have an experience in the field of sight which is identical with that of rhythm in hearing so far as all the essentials of the experience are concerned. What we are dealing with is a specific psychological experience, described somewhat as I have attempted in the first part of this paper. It is not a mere analogy to sound rhythms, but a rhythm just as complete and direct as the rhythm we are familiar with in listening to the beats of a metronome. I shall treat of several phases of visual rhythm, especially its relation to kinæsthetic sensations. Involuntary and voluntary light rhythms in the forms and limitations of grouping will be shown to parallel partially the auditory perception of rhythm. So far as I am aware this is the first psychological work that has been undertaken in the grouping of light flashes. It is hoped that it may form an introduction to the field of visual rhythm, which offers numerous interesting side lights on this characteristic mental experience. As an introduction it can claim only that completeness which attaches to a preliminary investigation.

The subject of visual rhythm may be said to have been approached in a way from another side, where the investigator was dealing, not with series of lights succeeding each other in

[1] 'Untersuchungen über d. Endorgan d. Nervus octavus,' p. 294. Cited by Scripture, 'Elements of Experimental Phonetics,' p. 527.

[2] Cited by Meumann, *Philos. Stud.*, X., 267.

time, but with the grouping impression which we get from viewing a surface covered with dots, lines or figures. Smith contends that we can get no experience of rhythm from such visual stimuli as they exist beside one another in space.[1] Meumann would admit an indirect experience of rhythm from viewing architectural effects or artistic designs. He says: "Kurz, nur soweit räumliche Gebilde dem betrachtenden Subject mittelbar Veranlassung zu periodisch succedirender Betrachtung geben, können sie etwa Antheil nehmen an den rhythmischen Erlebnissen."[2] He considers that rhythm rests upon certain periodic alterations of attention and, therefore, rightly asks whether these alterations occur in the same way in case of viewing an artistic design when like elements appear as grouped. What we wish to know is whether the experiences are the same in quality, not whether the causes of stimulation exist at the same time or follow each other in objective time. It may be enough that the subject gets the stimuli successively by turning his eyes from one part of the picture to the next. For this reason it seems to me that Smith's argument against the possibility of what we may call artistic visual rhythm seems to be insufficient.

She speaks of the matter in criticising Billroth's conception of symmetry as 'resting rhythm.'[3] She says: "Das Wort Rhythmus bezeichnet dabei nicht mehr jenes charakteristiche psychische Erlebniss, welches der Mensch bekommt durch seine eigene Bewegung und durch Geräusche, die das Gehörorgan afficiren, sondern ein räumliches Nebeneinander."[4] To be sure there is a spatial adjacency of the elements in symmetry, but that is not the reason why symmetry is not a rhythmical experience. It is rather because here the actual experience of grouping is lacking. There seem to be good reasons for believing that the perception of groups among repeated decorative figures, lines, etc., is a real rhythmical experience depending upon the repetition of a like accompaniment of strain sensations. Schumann, for example, is positive that the groups appearing on a surface covered uniformly with dots are really

[1] *Philos. Stud.*, XVI., 200.

[2] *Philos. Stud.*, X., 262.

[3] 'Wer ist musikalisch?' *Deutsche Rundschau*, Oct., 1894, Sept., 1895.

[4] *Philos. Stud.*, XVI., 300.

perceived as units.[1] He explains this unitary character on the ground of attention and does not attempt to correlate it physiologically. In so far as these groups are units merely in the sense that any object because perceived at one time is a unit, we have no rhythmical quality in them to explain; but if the groups are really limited in complexity, as he claims, it would seem that the grouping depended upon some other factor. Schumann says of himself, when he views a field filled with small black squares, 'for complexes, which consist of more than sixteen elements, an indirect reproduction is no longer possible.'[2] Below that number of elements he finds grouping in various forms arising quite involuntarily. The fact that we may have to turn our eyes from one field to the next does not seem to prove that we do not get an experience of rhythm. The feeling of rhythm may be independent of this necessary turning of the eyes to get the next stimulus. If we turned our head after each sound of a metronome we would hardly say we were no longer getting a direct experience of rhythm. Moreover, the grouping of dots may arise within a single field of vision. If these groups have a distinct quality, something more than the logical conception of their identity, that quality probably depends upon some sort of accompanying kinæsthetic sensation. I am inclined to think that here too we have a rhythmical experience in vision, although it is much more difficult to trace out any correlate for the group feeling than in the case of serial light flashes.

2. Method and Apparatus.

In arranging my experiments it was desirable to be able to give uniform lights at regular intervals and to change the intervals, the duration or the intensity of the lights. It was also necessary to produce the flashes in a form that would reduce, as much as possible, the fatigue effects that accompany such experiments. At first I tried giving the flashes by revolving a disk containing openings in front of an electric lantern or of

[1] 'Beiträge zur Analyse der Gesichtwahrnehmungen,' *Ztsch. f. Psychol.*, XXIII., 25.

[2] *Op. cit.*, p. 26.

an incandescent light inclosed in a box. This is quite satisfactory, except that the flash is let on or shut off the field of vision by the moving of a shadow (the edge of the slit in the disk) across the screen. To obviate this appearance of movement across the field the method was changed and the lights produced by sending an electric current through an incandescent bulb for a small fraction of a second. The bulb was inclosed in a light-tight box and the light allowed to shine through a circular opening 4 cm. in diameter. This opening was covered with a piece of ground glass to diffuse the light uniformly. The subject did not look at the lamp, but at the flash as it was thrown against a white wall in front of him. The box containing the electric bulb was placed between the subject and the wall. It was slightly to the side, about three feet from the floor, with the light opening, of course, turned away from the subject. The flash, as it appeared on the wall, was about 50 cm. in diameter, brighter at the center and fading out toward the edge. This kind of light proved to be on the whole the best that could be devised. It was very much less fatiguing for the subject to look at the flash on the wall than to look directly at the light. Daylight was excluded from the room. By lighting the room by another 16-candle-power light, after-images of the flash were almost entirely avoided.

The incandescent bulb which produced the flash was lighted in the following manner. A special electric-contact wheel was devised. When rotated by a motor connected with a speed reducer, this wheel gave contacts of long enough duration to heat the filament to incandescence. A duration of .1 sec. was sufficient to give a rather faint flash. Under the speeds usually used the contact continued from .2 to .3 sec., which was considerably above the time necessary to heat the filament of the lamp to its maximum. A weak current from a single storage cell passed through the contact wheel. This instrument was in the circuit of a relay. The high potential current which lighted the lamp was made and broken by the relay simultaneously with the make and break of the contact wheel.

The contact instrument (see plate at the end of the thesis) consisted of a wheel of hard rubber, 22 cm. in diameter and 2

cm. thick, which was rotated on ball bearings. The contact was made by a flat brass spring (*S*), 15 mm. wide, resting on the rim of the wheel and touching at certain intervals a brass contact-piece (*C*), set into the edge of the wheel, level with its circumference. The brass contact was turned to exactly fit a groove in the rim of the wheel, 8 mm. square, which ran entirely around one edge of the circumference. The contact-piece was held in place by a bracket which caught into another square-cut groove on the flat surface of the wheel. This bracket was made to fit so snugly that it would hold in place without a screw. At the same time the contact-piece could be removed and placed at any point on the circumference desired. A scale of degrees was marked on the wheel to assist in this adjustment. The contact-piece was connected with the axle of the wheel by an easily-removed copper wire running to a metal plate at the center of the wheel. The current passed through the axle and out through one of the posts of the support. The noise of contacts was eliminated by having the spring contact constantly touching the rubber rim of the wheel, only part of it extending over the groove containing the brass contacts. The ends of the brass contacts were tipped with hard rubber to assist in preventing the noise of a click when the spring touched the contact. So long as the wheel did not rotate more rapidly than once a second, which was faster than any speed used, there was no noise from the contacts. This was a decided advantage, for it was then possible to have the subject in the same room with the apparatus. The only noise was the hum of the motor. This was constant and did not interfere with the experiments. So far as the purpose of this investigation was concerned, any errors in the duration of the contacts or intervals were so small as to be negligible. The smallest difference in the length of a .5 sec. interval which could be distinguished eight out of ten times was about .15 sec., while the average error of the interval, as tested by the chronoscope, was about .005 sec. The average error in the duration of a .26 sec. contact was less than .002 sec., while the smallest difference that could be distinguished by a subject eight out of ten times was about .015 sec. The speed of rotation of the contact wheel could be changed by belt-

ing to three different pulleys on the wheel or five different-sized pulleys on the last wheel of the speed reducer. This gave considerable variety in the rapidity of the lights, while the contacts could be shifted about the circumference of the contact wheel to further vary the intervals, if desired. For the experiments with subjective rhythmization two contacts 60° long were used. For objective rhythms, consisting of a long light and one or two short lights, one of these 60° contacts was removed and the short lights produced by 40° contacts. A change in the intensity of the flashes could also be made by intercepting a piece of opal glass in front of the lamp. In this case the box containing the lamp was placed behind the observer, so as not to disturb him. This of course enlarged the flash on the wall. The duration of the flashes and the length of the intervals will be stated in connection with each of the experiments.

There are many complications, physiological as well as mechanical, which are encountered in working with lights, that are not found with sounds. In the first place it is impossible to produce a light which will appear stationary. Even if a light the size of a pinhole flashes at intervals, it will seem to swell out from the center and then contract. The flashes on the wall had the same appearance. The explanation of this phenomenon seems to be physiological. Physically the flash covers the entire surface at the same time. Psychologically it seems first to strike more intensely at the center and afterward to spread uniformly with a gradual decrease in intensity toward the diffused circumference of a circle, where it fades into the light of the surrounding field. This appearance is reversed as the flash leaves the wall, the intense light at the center remaining to the last. This effect might be explained as due either to a central or a retinal process. We might suppose that the part of the retina on which the center of the flash falls is first excited, because stimulated by the most intense light rays. The less intense rays, although reaching the retina at the same time, take longer to overcome the inertia of the rods and cones on which they fall, and thus come later to consciousness. On the other hand, the entire appearance may be a matter of interpretation in the higher brain center. The currents from all parts of the retina

reaching the center of sight at the same time, we become first conscious of those which are most intense. The effect of this appearance of movement on the question whether we really have a visual rhythm I shall treat later in discussing the similarity of visual and auditory rhythm.

Another circumstance in connection with lights, which it seems impossible to eliminate, is that the light of longer duration always seems to be of greater intensity, at least within the limits of duration within which we can get a rhythmical grouping. By rotating a disk with different openings in front of an electric tuning fork it is possible to get sounds of different duration which still appear of the same intensity.[1] My experiments indicate that this is not possible with lights. The work of Lough shows that when the light is below its physiological maximum the intensity appears to vary directly with the duration of the stimulus.[2] Exner found that it takes .3 sec. or less for a light stimulus to reach its maximum. After that it loses in brightness from fatigue, so that at the end of .6 sec. it is only 70 per cent. as bright.[3] But this loss from fatigue is not the only factor to be considered, for I found, with lights seen in succession, that one which endured 3 sec. was invariably regarded as brighter than one which endured 1.5 sec. Longer duration thus seems to have a greater effect in interpretation than fatigue after a light reaches its physiological maximum. The greater quantity of light causes the subject to think it is brighter. Under such conditions it is apparent that similar psychological effects are produced by giving long and short light flashes as by giving bright and dim flashes. It is impossible to study the effect of an increase in the duration of a unit in the light group independent of an apparent change in intensity.

3. The Subjective Rhythmic Impression from Like Flashes of Light.

The question whether a rhythmic impression is obtained from a series of like flashes of light occurring at regular inter-

[1] Bolton, *Amer. J. of Psychol.*, VI., 229.

[2] 'The Relation of Intensity to Duration of Stimulation in our Sensations of Light,' *Psychol. Rev.*, III., 484–492.

[3] 'Ueber die zu einer Gesichtswahrnehmung nötige Zeit,' *Sitzgsber. d. Wiener Acad.*, 1868. Cited, Hermann, 'Handbuch der Physiol. der Sinnesorgane,' pp. 219, 224.

vals is to be determined by introspection. For my experiments I had twenty-six subjects, half of whom were completely naïve, not even knowing the topic of the research. Three of the subjects were women. All were advanced students at Columbia University, the majority having had considerable experience in introspection. We will first consider the introspections of naïve subjects before any suggestion had been given them of rhythmic grouping. This bears upon the question whether an involuntary rhythm is ever obtained from uniform lights. When beginning an introspection with a new subject extreme care was taken not to ask any questions which would suggest rhythmic grouping, or to have anything in the conditions surrounding the experiment that would bring up the idea of grouping. Later in the introspection, when no observation regarding rhythm was made, I asked suggestive questions, had the subject beat time to the rhythm, and in other ways endeavored to see if the impression of rhythm could be developed. For the introspections as to rhythm two different rates of lights were used. It was found that the subjects preferred not to have the flashes come as rapidly as .3 sec., which is considered a favorable speed for sounds.[1] The favored rate for lights seemed to be about .7 sec. apart, although some of the subjects preferred .5 sec. and were given that rate. The durations of the lights with these intervals were .3 sec. and .25 sec. respectively. These rates were chosen after experimenting with subjects who readily perceived rhythmical grouping.

a. Involuntary Rhythms.

The first direction given a naïve subject was: 'Describe your feelings as you look at the lights.' Subject 1, male, non-musical, at once noted what might be termed the accompaniment of a rhythmical impression. He said: "It gives me a feeling of suspense. I don't know whether you care about it, but for anything rhythmical like that I have a tendency to breathe for every three." Subject 9, female, musical, said: "There is an idea of rhythm in it, sort of swinging in the way it comes." (Question —

[1] Meumann, *Philos. Stud.*, X., 302. Squire, 'A Genetic Study of Rhythm,' p. 85, says between .3 and .6 sec.

What do you mean?) "Same idea as rhythm, perfectly regular, a pendulum idea, movement." At the next direction to the subjects, 'describe how the lights seem,' nothing else being said, Subject 9 continued: "*It is more intense sometimes than at others. Every other one that way, every other one more intense.*" The next direction to all subjects was: 'Describe the intervals.' This same subject, No. 9, said: "I think they are regular. No, they are not. Every other one is shorter." (Question — Do you feel quite sure?) "Yes, they appear that way." The above statements of Subject 9 taken with her subsequent introspection convince me that this subject perceived a very definite subjective grouping of the lights without any outside suggestion. Her attitude and manner of utterance, as well as her discussion of the matter afterward, made it quite certain that the rhythmic impression had arisen involuntarily, in the same way that listening to regular sounds gives rise to a grouping experience without conscious attempts at rhythm. The above statements are just what we would have expected if she had involuntarily grouped the lights. Too much emphasis can hardly be placed on the fact that no suggestion up to this point had been given the subject to group the lights or look for any regular changes in them.

With the next question we find Subject 1 more definitely noting a grouping. The question was: 'Do the lights and intervals always seem the same?' No. 1 said: "Some of the lights are more distinct. I haven't noticed whether that occurs at regular intervals. I couldn't say. It doesn't seem so." (Question — How about the intervals?) "They do seem much alike, but are very much affected by a tendency that has grown in my mind to count the lights in fours. They seem to be divided into sets of four. After every fourth is a better defined interval." (Question — In what way?) "The last light does not seem to fade into the next as the others do. The shadow seems to cut off so that the first of the next four seems to start more clearly defined." This statement, considered with the previous observation of this subject that he was breathing to every three lights and his comparison at once of the lights to 'the long rhythmical flash which seems to rise from the horizon at

sea into the sky,' indicates that No. 1 probably also involuntarily perceived a grouping. Of the 13 naïve subjects No. 6 is the only other one who can be said to have approached a grouping without outside suggestion. My next direction contained a direct suggestion of grouping. It was: 'Count the lights in twos to yourself. Do the lights and intervals always seem the same?' Subject 6 then remarked that he had noticed the grouping when he counted in twos before I had suggested it. He said: "Of course, if I count them in twos they seem to group in twos. The two come nearer together. At the same time, if I set my attention on the lights this disappears. Sometimes the first is brighter than the other. There is no regularity as to brightness. I might say that I had just noticed this before you asked the question." It is to be noted that both Nos. 1 and 6 grouped first without any accented light, but feeling the groups set off by a longer interval. I shall discuss this simplest form of rhythm later.

b. Voluntary Subjective Rhythms.

May the rhythmic perception of lights be developed in subjects, even when it is not at first noticed by them? To determine this question I tried two different methods of procedure. The first was to ask the subjects to count the lights by twos, *e. g.*, '1, 2, 1, 2, 1, 2,' and tell me if they noticed any difference in the lights or intervals. If this was unsuccessful I asked them to beat time with a baton to the lights, allowing them at first to choose their own method. When this did not give a visual grouping, I had them vary their beating so as to get a distinct group movement for accompaniment to the lights. Of the 20 subjects whom I carried carefully through this procedure in the same manner, 11 obtained a grouping impression from the lights at the first suggestion of counting. The others obtained the visual rhythm only when the counting was accompanied by beating time with the baton or some part of their bodies. With several the rhythm was uncertain and seemed to keep up for only a short time. Except the three subjects noted under involuntary rhythm, the impression was of course developed by indirect suggestions of counting or movement, although in all ex-

cept one or two cases the subjects observed the rhythmic impression themselves without my telling them to try to group the lights. The subjects would frequently say: "I feel as if I am making the lights appear in groups." In every case, however, the accompanying tension or movement was undoubtedly carried over into an apparent change in the light series itself. Among the more rhythmical subjects the feeling that the lights were really different often became very strong. In such cases the subject lost the feeling that he was grouping the lights; the impression became what might be termed an involuntary rhythm.

Difference in Subjects.

Almost the first thing that is to be observed in experiments with rhythm is the great difference in subjects. I suspect that this may be traced to a fundamental difference in the attitude of people during experimentation. To the subject who turns out to be the most rhythmical, the lights appear as interesting stimuli the effect of which on himself he tends to watch and give way to. To the other class of subjects the flashes are specific units which are to be carefully judged and compared. Any apparent difference is at once to be severely weighed and criticised. A few instances will illustrate this difference among my subjects:

Subject 9, who perceives a light rhythm involuntarily, when I gave her an objective rhythm in which each alternate light was .1 sec. less in duration (very noticeably dimmer), grouped with the accent on the dim light and for some time called it brighter. She repeated this again a little later before I told her that she had reversed the objective difference in the lights. No. 1, another rhythmical subject, said regarding his attitude: "I have just let my mind go as sometimes when looking at a wall. I have not tried to be at all critical." On the contrary Subject 12 said: "I can imagine how an accented light would look if brighter, but it does not seem that way. I think that the real trouble is that I am wondering how they are." Subject 6: "As soon as I put my mind on the lights, I judge them." Subject 5: "I have cultivated a habit of letting things impress me, not trying to see how I impress them." Six of the subjects, when told to beat time to the lights, at once made one beat to each light. This put them in a critical attitude in which all the lights and intervals seemed the same, although at other times they perceived rhythms. Two other subjects thought that the lights seemed more irregular when they beat time the same way. From their critical nature some subjects at once found it almost impossible to consider an appearance of rhythm as soon as they had convinced themselves the lights were alike. Subject 21, for example, said when counting in twos: "They

are alike as two peas. My conviction is that those lights are just alike and come at perfectly regular intervals. If I were to expect things to be alike and they were as near alike as those lights I should be satisfied. I should think I made up my mind that the lights were alike and afterward believed them alike." The same subject when listening to the beats of a metronome and asked if he could make them seem in groups of two said: "The time, intensity and tones of the beats seem alike even when I beat time. I can't honestly say that there seems any difference in the sounds." Subject 13 was of the critical type who found it practically impossible to assume a passive attitude. His introspections seem to flatly contradict themselves at times, so that I am not convinced he obtained a rhythmic impression from either metronome beats or lights. He said concerning the metronome beats .3 sec. apart: "I naturally group them in fours. I studied music when a child. It was 1, 2, 3, 4 and I have never gotten over it. The interval between does not seem different. I just count four and start again. I think I see what is meant by a rhythmic impression and I do not think I get it. I can sit here and imagine that the sounds or lights appear grouped; but, if I just notice them, they do not." At another time he said that, when he imagined the lights in groups with a longer interval between the groups, they did appear that way while he looked at them.

Movement and Tension Sensations.

Serial flashes of light give an unusual opportunity to study the perception of rhythm in a field where few subjects have experienced it before. We can see the rhythm in its making. For this reason it seemed that valuable suggestions might be gathered touching the general problems of rhythm, especially its genetic development. The effect of movement and strain sensations is one of the most important aspects to be noted. First let us notice the impulse to a motor reaction with lights. It is a frequent observation that sounds in series tend to make us move. With a series of like lights we find the subjects saying:

Subject 7: "The light gives me a sort of swaying sensation, as if I wanted to go with it. My head goes back as the light comes on and forward as it goes out." Subject 14: "I have a sort of contracting feeling which seems to rise with the light, a drawn feeling when it is out." Subject 1: "I seem to find almost a physical response to the lights. I get almost as if I expect something to happen." (Note the connection of the physical reaction and expectation). Subject 8 found that the light stimulated bodily movements more than the metronome beat. He said of the latter when beating at the rate of 184 per minute: "There doesn't seem to be any bodily reaction to amount to anything with this as compared with the lights. The light is exciting, while the sound is not at all. The light makes me move my head and vocal organs, makes me tense all over, especially my head and throat." Subject 18, when beating time: "It seems as if the light makes my hand go. Just as if a fellow whistles a tune, the

light commands." There were numerous other observations of a like nature, but these illustrate sufficiently the tendency to move.

More important to note is the movement or straining of muscles directly associated with grouping. These corroborate, I take it, as much as it is possible for introspections, a kinæsthetic explanation of rhythm, such as I have defended in Part I. That muscle changes cause the illusion of grouping is what the following introspections indicate. It is perhaps their chief value:

Subject 15: "The lights do not seem to make a group unless I breathe or move in time to them, or do something." This subject had been uncertain about a rhythmic impression until be began to beat time. He then said: "In the last four pairs, one seems to be brighter. That is persisting pretty well too. I think I was accenting the first with my movement." Later with a group movement to three lights he said: "They are very decidedly coming in threes. The end of the three seems to chop off a series and the next is decidedly beginning a new series, although the lights seem alike in brightness." (Note that the groups were set off by intervals without accent.) Subject 11: "I do not think I ever get the impression of rhythm if I stop all movements, especially, in my case, with the tongue; nor do I think, if I could hold myself perfectly rigid, I could get rhythm from sounds." Subject 17, when grouping: "I feel as if I was getting into a sort of swinging movement, which becomes a tension if I inhibit it." When making a circular movement include three lights for each revolution of the baton, he said: "The movement requiring the most effort seems to emphasize the light. It then persists longer." Subject 14 first noticed a rhythmic grouping when she was nodding her head to three lights. Almost the first thing noted in beating time to lights is that a forcible movement seems to make the light appear brighter. Subject 10: "The second of the two seems brighter; the movement to that one was more emphatic." Subject 1: "The down beat on which I bring my emphasis seems to accompany the light which is most clearly defined, which is marked plainer on the wall, more intense and larger." Subject 4: "I can make any light seem brighter by emphasizing it, by putting an accent on it." (Question—In what way?) "I strain myself, my breathing, my chest." Subject 22: "I found myself taking a relatively strong inhalation on the accented beats and short exhalations on the others. The breathing was not abdominal, but was done by raising and lowering the chest. There was a slight tendency to raise and lower the whole upper part of the body. The raising always marks the accented beat." Rhythmic changes in the lights which follow quite specific changes in movement are illustrated in the following introspections: Subject 2, making a continuous movement in one direction until two lights had appeared: "I only seem to see one light. The two lights seem to fade into each other." Subject 3: "If I move the baton one way to two flashes and then change, the second flash seems to follow more quickly after the first." Subject 4 did not get a rhythm from beating to each light, but when he made a circular movement for a group, then the light on the downward part of the movement seemed brighter. Subject 7, making an up and down beat to one group: "The up stroke seems longer and the light at the end of it seems far-

ther away than on the down stroke." Subject 9, beating a stroke in a different direction to each of the lights in a three-group, found that the longest interval agreed with the longest movement which came before the first light in the group. Subject 6: "When the lights are coming at about the rate at which I breathe that makes a decided difference. When there is an expiration and taking in of my breath to two lights, that makes a more decided rhythm of two." Subject 10 noted that the first movement seemed to be made with more effort and the third movement was light and easy. This agreed with the intensities of the lights as they appeared. Subject 12, who had previously obtained no rhythmic impression, when he came to make a movement covering a group of lights said: "Oh, they are in little bunches. It seems as if I made one beat and then two closer together and get a sort of rhythm of threes in the lights." Subject 19, making a down beat to one and up beat to the next light: "When a person moves the baton up the second light, coming at that stroke, seems to sort of hang fire. The first goes with a sort of snap and the second slides off. The light seems to follow my beating." Subject 20 obtained only one form of rhythm, even with the metronome. It was a three-group in which he made a down movement of the baton to the first and an up movement to the next two of the group. The two on the up beat then seemed to be 'closer together' with a longer interval before them. Subject 14, making a beat in one direction cover two lights and then changing to the opposite direction, noticed that the change in movement made the interval after the second light seem lengthened.

That the grouping impression was not due to a retinal fatigue is shown by observations of quite real fatigue effects on the part of many of the subjects, when first asked to describe the lights. The introspections prove that this was very different from the rhythmic grouping effect. Here are a few examples of fatigue taken from descriptions of the lights:

Subject 5: "The lights seem to increase four or five to a maximum and then decrease. Sometimes there are two or three bright and then only one." Subject 8: "Sometimes I don't see them as well as others. About every fifteenth it seems to disappear." Subject 12: "At times the flashes are pretty definite in outline, then more diffuse. Now they are quick and small; now growing bigger." Subject 15: "Some flashes appear to be brighter; about every fifth appears brighter." Subject 20: "Some seem to be quick and short. After I counted eight, it seemed as if the next four were quick." Subject 19: "The flashes sometimes seem to get quicker and then go slower."

Appearance of the Group.

When it comes to describing the rhythmic impression as the subjects obtained it from the lights, we find at once two characteristic types, accented and unaccented groups. The simplest perception of the group is with no apparent qualitative change in the lights. The units in the group seem crowded closer together and a longer interval appears before the next group starts. Of

the twenty subjects there were seven who only obtained this type of grouping, or else obtained it most easily, finding it difficult to get the appearance of an intensive difference in the lights. Furthermore, the accented grouping was in every case but one accompanied by an apparent change in the interval between the groups compared with the intervals within the groups. Subject 15 reported an accented rhythm which to him seemed to show no difference in the interval between the groups. If we consider that visual rhythm is practically a new experience, we may say with some assurance that it is most easily developed in the form of an unaccented grouping. This indicates that the unaccented group is genetically the most primitive. It agrees with the observation of Squire, previously cited, that the spondee is the earliest free grouping among school children repeating the syllable 'me.'[1] The tendency to perceive at once an accented unit within the group is, however, very strong. It is increased of course by our associations with music. The accented group appears almost always with either the first or last light seemingly brighter, sharper or otherwise emphatic. The most rhythmical subjects are able with ease to voluntarily change the position of the accent or the rhythmic form of the group. Statements from the subjects themselves give the best idea of how the visual group appears:

Subject 8: "I have a tendency to move my head and expect the first light of the group. If I start to expect it I can make the time before it seem longer. The one accented seems brightest and causes more physical commotion. I seem to be ready and waiting to say it. The other just seems to come along as part of a whole. I don't have to bother about it at all." Subject 7: "I can make the group seem closer together. The accented flash seems to jump up quicker. I can put the accent where I please after a little time, but not right away." Subject 15: "When I think of the lights in bunches of three there seems to be a sort of bracket around them. I don't seem to look at the interval after the three. I get to three and then start in again at one, so that that interval does not appear." Subject 2: "Every third seems shorter. The first seems most bright. Between three and the next group the interval is longest." With a two-group she said: "The first is always stronger than the second." Subject 17: "You can get nearly any rhythm you want. If I take an indifferent attitude, the second light seems brighter. If I start with making the first bright that will continue. Or I can make a difference in the duration of the lights; one will stay longer. Between the groups is a longer interval. The more emphasized light seems to spread out farther and be stronger. I can have my attention on

[1] *Op. cit.*, p. 50.

something else and yet the grouping continues. The two grouping I think may become entirely involuntary with me. There seems to be a rest after each group." Subject 11: "Every other one is brighter, larger, more intense. When I remain quite passive there seems to be a shorter interval between lights one and two, counting in twos." Subject 22: "At first I thought I could make the alternate ones seem longer. Then the one I attempted to accent seemed heavier, rather than longer. I finally concluded that I had a feeling that one of the intervals contained more time, not because it was longer in duration, but because the time hurried along faster. I thus perceived more time in the interval. I tend to see time in strips and thus measure it in special terms. I also noticed that when I perceived a two-rhythm the entire group was perceived as a unit, the interval between the lights forming part of it, so that the duration of the lights themselves had little effect."

The pleasure-pain effects of the light series varied considerably. Subject 7: "At first it was sort of startling. Now it is pleasant, sort of soothing effect. It tends to make me dreamy." Subject 11: "It is pleasurable; rather exhilarating." Subject 7: "It is monotonous." Subject 5: "They don't irritate or tantalize me." Subject 2: "They make me mad, because of the associations with the metronome."

Produced Visual Rhythms.

Early in the introspection of each subject, in order to test how strong the tendency to visual rhythm was, I arranged a plan by which the subjects by pressing slightly on a noiseless key could produce light flashes at intervals to suit themselves. The lights remained on the wall as long as they held the key down. The key was an ordinary telegraph key, made practically silent by limiting its play to a fraction of a millimeter. The lights were produced by connecting the key in the low potential circuit of the relay. Each pressure of the key drew over the armature of the magnet and allowed the high potential current to pass through the incandescent bulb. The subjects were told to produce a series of lights in the way that seemed most agreeable to them. This test was tried before the subjects had tried beating time to the serial lights. Only one person who had not obtained a rhythmic impression from the lights previously, seemed to develop the impression here. He said: "There immediately came into my mind special melodies which I might beat. There was a feeling of curiosity as to how a song you are familiar with would look." I observed that he

was grouping in threes and found that he had been keeping the rhythm of one of the popular waltzes. The experience of producing a light flash by a pressure with the finger was so novel that nearly all of the subjects contented themselves at once with seeing how regularly they could make the lights come and go. Six subjects out of the twenty produced rhythmic groupings of two or three lights. Producing flashes in any way agreeable to the subject I conclude is quite ineffectual in developing a rhythmic impression. If the subjects practiced producing the lights in groups this might aid the feeling of visual rhythm from an objective series of flashes. It seemed to do so with some who tried it.

Later in the experiments, after the subjects had obtained visual rhythms from the serial lights, I had them produce the flashes again, this time with the direction to make groups of two and of three. I then timed ten groups each way. From an average of the twenty subjects tested the groups were found to be the following lengths:

Two-group, 2.0 sec.; Av. error of Av., .1 sec.
Three-group, 2.4 sec.; Av. error of Av., .1 sec.

This close agreement of the lengths of the groups for two- and three-rhythms is in accord with the general opinion as to the rhythms of sound and of movement. The tendency in rhythm is to keep the group length about the same. If the subject had added a proportional time for the extra third light the group would have been 3 sec. long.

Limitations in Grouping.

Two other series of experiments, after the subjects had perceived visual rhythms, have a direct bearing on the nature of the experience. In one series the subjects were asked how many lights they could group when the intervals between were .7 sec. long. This was a fairly convenient rate for visual rhythm, the rate they were most familiar with. In the other series of experiments the rate of the lights was slowed to 1 sec. and the subjects asked if they could still group. If they could the interval was lengthened still more. These experiments indicate the limitations on grouping for flashes such as we

worked with. As to the number of units possible to group when the lights were .7 sec. apart, the subject was asked: "How many lights can you voluntarily keep in a group, holding the rhythmic impression so that you seem to compare one group with the next in return of the same feeling? What, if anything, is there in the rhythmic impression besides counting?" Five subjects were quite certain that three was the largest group which they felt rhythmically; five subjects named four as the limit; five named eight; two named five; one named six, and one named thirteen. Groups above three often broke up into smaller divisions, but the higher group still preserved its unity in the cases I have classified above. All the subjects tried higher groupings and several at times thought they obtained groups above these limits, one even naming forty as the limit. I shall discuss these large groups below. Some of the comments of the subjects show how real was the experience of grouping:

Subject 7: "Up to five I think I set the groups off just by muscular sense, without any counting at all. Above that I think it is only counting. The lights seem to group into threes almost involuntarily." Subject 8: "I don't believe I can group more than four. With eight there seem to be two groups of equal value. What broke it into fours was the fifth light flashing out more brightly. There is a tension mentally and physically before the accented. The others fall without effort." Subject 10: "I would almost swear as to the long interval at the end of the group." Subject 12: "I think I group thirteen but not more. With thirteen there is a certain sense of wholeness. Above that I begin to feel a discontinuity. The group feeling seems to be concerned with expectation, with how big a bunch of things I am going to group. When the group gets too big it begins to split." Subject 16: "With four it is easy enough. The members seem to run closer together in the group. With higher numbers I can get a division between the groups, but can't seem to keep the whole in mind. They are not so cohesive." Subject 17: "In voluntary grouping I have my attention on a kind of strain. I can group with my breath, little hitches in it." Subject 18: "There seems to be a sort of hazy light during the group and then clear between the groups."

One of the most interesting introspections is that of Subject 9, who thought she actually obtained a grouping impression from 40 lights. All groups above four lights broke into smaller groupings with this subject; but she still thought she held the higher group together. It is difficult to suppose that a group over 20 sec. long could be felt as repeating. Here is her introspection of the higher groupings: "I can go to 20. This is not nearly so good as 18 or 16. The last four lights become very disagreeable. I want to drop them." After saying that she grouped 40, I asked if there was more than counting and making a longer interval at the end. "Yes, I was drawing it out. I would represent my feelings

as a rather flat curve. It is like holding a note. It is an effort and unpleasant, but I think I hold them all together." When told to try 60: "There is a muscular sensation from breathing. I lost it a number of times. I had to take a breath and I would not in the smaller numbers. I think I can do 40, but I have to make all the lights in between less brilliant. I don't let them seem as bright. If I hold one breath over the whole 40 it seems more like a group." She held her breath thus to get the 40-group. She tried to get a rhythm by associating a continuous monotone with 60 lights, but said: "The monotone did away with the rhythmic swell. It makes it flat. So long as I can hold my breath I can get a rhythm. I can't hold my breathing for 60." Several other subjects thought at times they obtained rhythmic groups containing from 10 to 20 lights, but their introspections were too uncertain thus to classify their experience.

The facts brought out by the attempt to get long groups are of value. In every case, it is to be observed, the grouping experience seemed to reach limits. In most cases this was very early. Long groups appeared invariably separated by decided intervals. The lengthened intervals were often accompanied by conscious changes in muscular tension, or by movement. The attempt to make large groups caused the subjects to note how they grouped. So far as they were able to describe the process in any but figurative terms, this grouping factor was apparently associated with kinæsthetic sensations.

As to the interval which may elapse between the lights and voluntary grouping still be possible, I found that the majority of the subjects could not group lights 2.5 sec. apart. Three subjects grouped when the lights were 2.5 sec. apart and three when they were 4 sec. apart. One subject, the same who obtained the 40 grouping above, thought she could group when the lights were 7.5 sec. apart; but could not when they were 9 sec. apart. The introspections here, as to the grouping impression, were similar to those when many units were grouped. So far as I could judge on careful questioning the earlier impression of rhythm was absent at the limits I have described.

Similarity to Auditory Rhythm.

A direct comparison of the rhythmic experience obtained from lights with that obtained from sounds was made by each of the subjects. The fact that no subject traced any difference in the essential quality of the experience is the best evidence, it seems to me, that we are dealing with like experiences in the two fields. There are differences in distinctness, in forcible-

ness of the rhythm ; but these are differences of degree, not kind. The rhythm in lights is usually more difficult to experience at first and harder to maintain. A striking fact, already noted under the difference in subjects, is that the two people who obtained only a questionable rhythmic experience from lights had the same difficulty with sounds. All the other 24 subjects were positive in their statement that they did experience a rhythmic grouping in the series of like lights. For the purpose of an introspective comparison the subjects were asked to listen to the sounds of a metronome. This was first run at the rate of 120 beats per minute, about the speed of the lights; and then at the rate of 184 per minute, which is supposed to be very favorable for sound rhythms. When the subjects were asked to describe the rhythmic impression from the sounds as compared with that from the lights, the most suggestive statements were as follows:

Subject 2: "The changes with the metronome are more definite and fixed. With the lights I might believe anything: I feel as if they might seem different next time. However, I find that I can change the accent with the sounds, although I supposed I could not." Subject 3: "There is more rhythm than I get from the lights. There is more emphasis on the first of the group here than with light, although the two within the group come together in light as they do in sound." Subject 8: "So far as the stimulus is concerned the rhythm seems to be more in the sound. In the lights I seem to notice the rhythm more in my reaction. The rhythm is therefore more sharply defined with the sound." Subject 9: "I can change the accent as with the lights. The individual lights are more gradual, the sounds come out with more of a jerk." Subject 10: "The sounds give me a more business-like rhythm. The idea of rhythm comes out more sharply." Subject 11: "In case of sound I am less able to vary and control the rhythm. I am not sure whether this is not due to my being more in the habit of regarding sounds in rhythm and making movements to them. We are more accustomed to an auditory series than a visual. It took some time to adjust myself to an introspection of the lights." Subject 12: "I can keep track of the sound rhythm easier, when I once get it going. There is just the same sort of feeling as with lights in starting a rhythm." Subject 14: "Sounds grouped more easily than the lights and I perceived the accent almost without trying." Subject 16: "I can get rhythmic grouping from the lights, but it does not seem to stay that way. There is more irregularity within the group than with sounds." Subject 17: "The sound is more responsive to changes in tension that I make. If I try to change the accent I have to do it by pressing my tongue against the roof of my mouth, or something like that. I do the same with the lights." Subject 19: "You can make the beats seem to say 'tick tock.' I didn't associate words that way with the lights. With the lights I should not notice the rhythm without its being suggested to me."

Similarity between the visual and auditory rhythmic impressions is one of the main questions of this introspective study. Are they alike, or are visual rhythms intrinsically motor and not, therefore, in the field of sight? We must admit that no light can be seen without an appearance of movement, as I have already noted. Is it these apparent movements that are grouped, or are they secondary and unnecessary to the grouped impression? The fact that only two or three subjects paid any attention to the seeming swell and contraction of the flash in getting their group feeling indicates that it had nothing essential to do with the rhythm. Moreover, the closest observation of the subjects' eyes when they were grouping lights, failed to show any movement of the eyeball. The kinæsthetic reaction on the part of the subject, which I believe accompanies rhythm, was not, therefore, *started* by motor sensations recurring in series. It arose from the series of light sensations in exactly the same way as it seems to arise in the case of a series of sound sensations. Under such conditions I believe we are thoroughly justified in supposing the subjects were right when they said they obtained a rhythm from the lights, and that they used 'rhythm' with the same psychological meaning as when speaking of sounds. Rhythm in any sense order might be said to require a motor factor, but that does not require us to say we have no auditory rhythm. Besides the purely motor rhythm from repeated movements, we realize that the impression may arise from an objective series of stimuli. These may be addressed to the ear or to the eye or probably to the other senses. The rhythm from successive lights is no more motor than is the auditory rhythm, and it is motor in exactly the same sense.

4. Interval and Intensity Illusions in Objective Light Rhythms.

In the following experiments the subjects were shown lights in series but grouped objectively by intensity or interval changes. My chief purpose was to determine, if possible, any general subjective rhythmic attitudes which would be assumed toward such objective changes. For example, when all the intervals were alike, would the subjects uniformly feel that the time

before the bright light which began a rhythmic group was longer? Would the first of uniform lights appear brighter after a long interval which objectively separated the lights into groups? What, if any, main directions could be assigned to these subjective tendencies? These were some of the questions to be answered. The investigation was different in purpose from that which Meumann conducted in the time valuation of intervals bounded by loud and dull sounds.[1] He and others, when working toward an exposition of the time sense, gave their subjects two intervals for comparison. In my case a regular series was kept up until the subject fell into the rhythm, when he was asked to describe the group appearance, both as to intensity and interval differences. Meumann sought to find how time intervals were valued. I sought to find the nature of the group impression as carried over into an objective series.

Objective Intensive Accent.

The objective rhythm with a bright flash every second or third light was given in two different ways. Under the first method (*A* in the tables), the difference in the lights was produced by intercepting a piece of opal glass in front of the box containing the incandescent bulb, thus dulling the flash as it appeared on the wall. The other plan (*B* in the tables), was to use 60° and 40° contacts. This, of course, does not cause an objective difference in intensity of the lights, but has that effect on the subjects. The lights were actually different in duration by about .1 sec., but to the subjects they appeared as different in intensity, the longer always appearing brighter. (See my previous discussion of method.) The difference in the duration of the flashes was unnoticed as such. Under both methods the intervals were kept uniform between the flashes. Under method *A* the intervals were .7 sec. Under method *B* they were .45 sec. when the three-group was given, and .8 sec. when the two-group was given. These intervals were found suitable for easy grouping and introspection. The changes in interval were made for convenience in arranging the 60° and 40° contacts on the contact wheel. So long as a different rate

[1] *Philos. Stud.*, IX., 274 ff.; XII., 128 ff.

did not interfere with the introspections, it was not necessary to maintain the same rate throughout the experiments. The lights from the 60° contacts were .3 sec. long and those from the 40° were .2 sec. The 60° contacts were used also in method *A*. Twenty subjects were tested. They were asked after observing each series to note any apparent difference in the length of the intervals or, in the case of the objectively uniform lights, in the intensity of the lights. They were to group the lights first with the accent at the beginning of the group and then at the end. Any change in appearance, when the accent was thus shifted, was especially to be noted.

Considering first the two-group with one light accented by being apparently brighter, the subjects noting the various differences are tabulated according to their statements. The accented figure (1′, 2) indicates that the bright light was regarded in that position in the group when the subject gave the introspection tabulated below. *A* and *B* refer to the two methods described above for producing the apparent intensive changes.

	1′, 2		1, 2′	
	A	*B*	*A*	*B*
Long before 1,	11	17	10	9
Long before 2,	5	3	3	9
Equal,	3		2	2
No decision,	1		5	

One of two reasons may usually be assigned why an interval seems longer. These arise from two tendencies which have different force according to the individual. Apparently the subject may consider the groups set off by a change in interval (always a long or always a short interval between the groups); or he may always note an appearance of a long interval next to the bright light (either consistently before or consistently after it). This is brought out in the following comparison, which shows the number of individuals who pursued one or the other of these methods when they changed the grouping from 1′, 2 to 1, 2′.

	A	*B*
Long always between the groups,	7	8
Short always between the groups,	1	2
Long always after the bright light,	4	1
Long always before the bright light,	1	7
Intervals equal,	2	2
No decision,	5	

We see that under method *B*, which was most convenient for introspection, ten subjects judged the intervals according to some grouping scheme which continued under both positions of the accent. Eight under method *B* kept the lengthened time in the same position as regards the bright light, either always before or always after it. This would contradict any uniform group arrangement of intervals, such as a long interval always between the groups. Those judgments where the intense stimulus seems to determine the position of the long interval might be explained as due to some condition of the eye connected with the bright light. For example, the effort connected with the decided change in the adaptation of the iris to the bright light might increase the apparent interval. But no such explanation could meet the cases where subjects persist in lengthening the interval between the groups, at one time before and next time after the bright light. It seems best to interpret these cases as due to the rhythmical attitude of the subject. This attitude is bound up, I should say, with muscular strain or movement, voluntarily or involuntarily started. Changes in tension or movement correlate with the interval illusion between the groups.

The same conditions of illusion are followed through for the three-grouping in the following table:

	1′, 2, 3		1, 2, 3′	
	A	*B*	*A*	*B*
Long before 1,	8	14	11	11
Long before 2,	9	3	2	
Long before 3,			2	5
No decision,	3	3	5	4
Shortest before 3,	14	18		
Shortest before 2,	2			
Shortest before 1,	1			
No decision,	3	2		
No. 2 dimmest,	14	14		
No. 3 dimmest,	3	3		
No decision,	3	3		

It will be noticed that two supplementary questions were asked as to the 1′, 2, 3 grouping, viz., the shortest interval and the dimmest of the two like lights. The answers to both indicate very general tendencies. We can say with some assur-

ance that the dimmest light is the one after the bright light. This apparently has a simple physiological explanation in the fatigued condition of the retina after an intense stimulus. The other tendency, *i.e.*, for the interval between the two dim lights to be shortened, is not so easily explained. It appears to be part of a group form in three-rhythms which is very general. It is contrary to general experience for dim lights when not in a group to appear to follow more rapidly than bright. (See note at the end of this section.) This indicates that the appearance of quickness here is connected with a specific rhythmical attitude. We should therefore look for its explanation in the rhythmical muscular responses. Slight muscular responses from the dimmer light flashes might be interpreted as tripping off more rapidly and the effect read over into the series of lights. Easy movements, meaning rapidity to us in other situations, would here associate the idea of rapidity with the dim lights in an objective three-rhythm.

With the three-group we also find the same methods at work in naming the position of the longest interval, as we found with the two-group. This is brought out as the subjects change from the 1′, 2, 3 to the 1, 2, 3′ grouping. It is shown by the following table:

	A	*B*
Long always between the groups,	6	8
Long always before the bright,		5
Long always after the bright,	5	3
Irregular,	4	
No decision,	5	4

Here again, under the best method, *B*, we find 16 out of the 20 subjects pursuing one or the other course I have outlined. The method by which the long interval is always kept between the groups indicates undoubtedly a more rhythmical attitude on the part of the subject. The illusion there may be said to be due to the feeling of rhythm, while the illusion in the case of those who keep the long interval next to the bright light probably has nothing to do with rhythm. As one or the other attitude predominates in the subject, so will his decisions be. The rhythmic attitude is plainly indicated by remarks like the following, which several of the subjects made: "Lights

Nos. 1 and 2 are always nearer together, whether the first or second is brighter." The two tendencies may combine at times and then a more pronounced interval appears. This is shown by a remark of Subject 6 when the 1, 2, 3′ group was tried: "The long interval is now *decidedly* after the bright light." With the 1′, 2, 3 grouping he had been uncertain, but when he changed the accented light the tendency to lengthen after the bright light, as well as to lengthen between groups, came together. He then noted emphatically the appearance of a long interval.

A rough measurement of the illusion in the case of the rhythmic attitude gives some idea of its strength. The ability to make a dimmer light seem brighter by accent in a group was tested by a method similar to *B*, in which the apparent dimness resulted from shortening the light. The test was made by revolving a disk, containing openings, in front of an electric lantern. The four rhythmical subjects who were tried found that, when merely trying to judge the lights, they could distinguish eight out of ten times a difference of .06 sec. in the lights .35 sec. long. The difference appeared as a difference in brightness, the long light always seeming brighter. Giving these subjects a rhythm with intervals between the lights about .7 sec. long and equal, they were all able to carry over an objective difference of .12 sec. in the duration of the lights, *i. e.*, they were able to make the shorter light seem brighter. To test the illusion of interval and see what difference could be carried over and the short interval still seem longer, I changed the position of the 40° contact, method *B*, in the experiment with the two-group. I found, with four subjects tested, that a difference of .15 sec. could be made between the intervals when the lights were about .7 sec. apart, without the subjects noting any effect on their rhythmic grouping. They all considered the interval before the 'bright' light (60°) longer when it began the group, although it was actually .15 sec. shorter. This objective difference could easily be distinguished by observation, but had not interfered with the opposite interval appearance between the rhythmic groups.

The illusions due to rhythmic grouping in lights are apparently the same as those that have been noticed by other observ-

ers for sounds. They include the lengthening of the interval between groups, the intensive accent, and the shortening of the time between unaccented units in the three-group. MacDougall, for example, pays especial attention to the lengthening of the group interval when listening to objective rhythms of loud and dull sounds.[1] Bolton notes the same thing in subjective grouping of sounds.[2] Squire shows the intensity and duration relations in speaking rhythms to be similar to those noted. She finds the end of the group marked by the longer pause.[3] Similar effects of rhythm on intensity and pauses are found in tapping rhythms or beating them on a drum.[4]

The rhythmical attitude toward the light series I have attempted to distinguish from that attitude which tends to place the long interval always next to the bright light. This latter attitude I have regarded as non-rhythmical under the supposition that it might be due to some peripheral change. To further test this hypothesis I tried the following experiment. A bright flash was brought into a series of uniform dull lights at an unexpected time. The intervals between the lights were .7 sec. as before. The subjects were told to notice any difference in the appearance of the interval or the light that followed the bright flash. The bright flash was produced by removing the opal glass from in front of the incandescent bulb. The introspections of 17 subjects as to the units after the bright light were as follows:

Next Interval.		Next Light.	
Longer,	12	Dimmer,	14
Shorter,	3	Brighter,	2
Alike,	2	Alike,	1

This strong tendency to lengthen the interval after the intense stimulus agrees with the observation of Meumann, who tried a similar experiment with a sound series. He introduced a loud hammer stroke into a series of dull strokes. Meumann suggested that the reason why the interval after the loud sound

[1] 'Harvard Psychol. Stud., I.,' *Monog. Sup. Psychol. Rev.*, IV., 378, 381.

[2] *Amer. J. of Psychol.*, VI., 204.

[3] *Op. cit.*, p. 52.

[4] MacDougall, *op. cit.*, p. 362. Miyake, *Stud. from the Yale Psychol. Lab.*, X., 15 ff.

was lengthened was because it began a new measure after a pause.[1] MacDougall found an opposite result in his experiments with a sound series. He says that "The influence of the introduction of such a louder sound is to cause a decrease in the apparent duration of the interval which follows it, and an increase in that of the interval which precedes it."[2] There can be no doubt that for the series of lights as used in my experiments the tendency was strongly, as Meumann found it, to lengthen the interval after the intense unit. The discrepancy with MacDougall's results with sounds may have been due to the difference in the quality of the stimulus or the length of the intervals. The explanation given by Meumann for this illusion in the interval does not seem sufficient. The lengthening could not be due, as he suggested, to a rhythmical attitude. A grouping would hardly be brought out suddenly by the introduction of a single bright light. If it were, the group would naturally begin with the bright light; not with the next light, as he thought. The fact that about a quarter of my subjects held to this tendency to lengthen the interval after the intense unit, in spite of the rhythmic tendency to lengthen the interval before the group, when the arrangement was 1′, 2 or 1′, 2, 3, indicates that the result may be due to some physiological condition in the sense organ, preceding any rhythmical reaction. Whether this condition is an adaptation of the pupil of the eye, as I have suggested, or some other peripheral change, my experiments would not determine. The point I wish to emphasize is that we have two distinct phenomena, one of which seems to require a rhythmical explanation and the other not. The dimming of the first unit after a more intense stimulus, which is shown in the table above and in that for the 1′, 2, 3 group, suggests a peripheral explanation at once, rather than a rhythmical. It may be explained very simply as the result of retinal fatigue from the intense stimulus. It seems likely, therefore, that with subjects to whom the interval after a bright light is always lengthened and the next light dimmer, the effect is not due

[1] *Philos. Stud.*, IX., 276.

[2] 'Harvard Psychol. Stud., I.,' *Monog. Sup. Psychol. Rev.*, IV., 364.

primarily to any rhythmical grouping, but to a peripheral change.[1]

Objective Interval Grouping.

When uniform lights in a series are set off into groups by a long interval after every second or after every third light, we have a different kind of objective rhythm in which to test the effect of subjective grouping. In these experiments the lights were .3 sec. in duration and separated by intervals of .7 sec. The groups were separated by a double interval. Here we find a great variety of rhythmic and judgment attitudes assumed by the subjects. They differ so much that classification almost becomes enumeration. The subjects were asked to describe any apparent difference in the lights within the objective groups. I have attempted to arrange the introspections in tabular form. The table ought not, however, to be interpreted as emphatically as tabulation makes it seem. In many cases the subjects were quite uncertain in their observations. The apparent difference in the lights was comparatively slight. The main result of the experiment was to demonstrate the great variability in attitudes. The number of subjects is given whose introspection was approximately in the form stated.

	2-Group.	3-Group.
Any light brighter, depending on how I accent,	6	4
None brighter, or not sure, even when accented,	2	2
First brighter naturally, without seeming to accent,	6	2
First dimmer naturally, without seeming to accent any,	0	3
Last brighter naturally, without seeming to accent,	2	2
Only the first would seem brighter when I accented,	1	0
Only the last would seem brighter when I accented,	3	6*
Only the middle would seem brighter when I accented,	0	1

* With three of these the last always seemed brighter even when they thought they accented the first. With the others the lights all appeared alike if they accented other than the third.

[1] Comparing a series of bright lights with a series of dull at the same rate, the dull produced by intercepting the opal glass, I found the tendency was for the brighter series to appear faster. Meumann obtained the same result with a few subjects when comparing series of greater and less intense induction sparks produced inside a sound-proof glass case. He also found that a louder sound series appeared faster. (*Philos. Stud.*, IX., 274 ff.) My results indicate that the illusion is not so decisive as he implies. Of the twenty subjects I tested, nine thought that the bright series was faster, three that the dim series was faster, and eight noted no difference. Meumann explains the effect as due to

The divisions in the above table are not all mutually exclusive. Some of those who found one light naturally bright could make others appear brighter by trying. I have tried to place such subjects according to the emphasis of their introspection. Supposing that all who naturally found the accent in one place could change it, we have from thirteen to sixteen who would say that the apparent differences in brightness were largely controlled by them. The striking thing is, perhaps, that there should be seven with the three-group and four with the two-group who found this rhythmic difference in brightness apparently beyond their control. In these cases the tendency was nine out of eleven to brighten the last light in the group. This could hardly be a retinal effect, at least nothing like fatigue, for the long objective pause came before the first light in the group which should then appear brighter. It apparently must be due to some central association or reaction after the light stimulus reaches the brain. This again might be a varying muscular response. It seems safe to say that nearly all the apparent changes in brightness which appear when like lights are set off into groups by long intervals, are due to some rhythmical attitude; but that these attitudes vary decidedly among different individuals.

5. Summary.

Contrary to the prevailing impression, we have found that the experience of rhythm in the field of vision is identical in its essentials with that in the auditory field. Since the experience is novel, it is at first more vague than with sounds, but it becomes quite precise with practice. Although visual rhythm is less distinct, it is just as direct as auditory. The difference, so far as is noted, is in degree, not in quality. The experiments show rhythm to be subjectively experienced from a series of like lights. Twenty-six subjects found this to be true in some measure. Whether the group feeling may arise involuntarily

fusion, the more discontinuous series appearing slower. We might suppose the effect explained peripherally by the brighter lights causing more commotion in the retina and leaving shorter rest periods; or the effect might be explained centrally as the interpreting of a bright series to be more rapid on account of the individual lights seeming to swell out more suddenly.

is not certain, but the introspections of two subjects at least indicate that it may. Both auditory and visual rhythm seem to be illusions due to the muscular reaction of the subject, combined with the sensations from objective serial stimuli. Rhythms in sight furnish new evidence of this connection between the experience and muscular tensions and movements. Subjects are better able to introspect here as to the bodily correlate, because not habitually neglecting it as with sound rhythms. Their observations show that apparent changes in the lights correlate with differences in movement. The difference between subjects as to their inclination toward rhythmical perception is more pronounced than in the practiced field of sound. Visual rhythms show several marks of being real experiences rather than ideational creations. Among these may be mentioned the fact that grouping has somewhat narrow limits and tends to rather definite forms. Voluntary grouping ordinarily stops before the lights are 2.5 sec. apart. Nearly all the subjects find it extremely difficult to hold more than four lights together in a group. When produced by the subjects, a group of three lights tends to the same length as a group of two. For the apparatus used this group length was about 2 sec. Favorable rates for grouping were found to be .5 and .7 sec.

The rhythmic experience in vision is fruitful for study, not only because it allows us to trace the varying forms and accompaniments to better advantage than in conventional sound rhythms, but also because sight rhythms are very suggestive in a genetic investigation. They show how the experience may be gradually developed by accompanying movements. In this development we find groups at first frequently perceived without accent, *i. e.*, set off solely by longer intervals. This may be regarded as a more primitive stage of the perception, a stage which some subjects find it difficult to pass beyond. The accented group, however, is most commonly experienced in subjective rhythmization. In this form the origin of the accent can usually be clearly traced to kinæsthetic changes.

The attitudes of the subjects toward objective rhythms indicate that apparent variations in intensity or interval may be due to either peripheral changes or rhythmic reactions. The length-

ening of the interval after an intense stimulus and the dulling of the next sensation are apparently of the peripheral order. An accentual brightness within the group, the lengthening of the interval between groups, and the shortening of the interval between the second and third units in a 1′, 2, 3 rhythm, are probably connected with the rhythmic response. These latter tendencies as to the form of the group are similar to those found in the field of sound.

PART III. MOTOR EXPRESSION OF TIME INTERVALS.

Rhythms and time are so intimately interwoven that I may be allowed to present here two series of experiments which bear upon the motor side of time problems. The first series studies the effect on the reproduction of time intervals when the standard is bounded by unlike stimuli, a sound and a light, compared with a standard bounded by like stimuli. The second series deals with the continued reproduction of the same interval and the correlation of the constant error of the individuals with their reaction time.

1. Intervals Bounded by Unlike Stimuli.

The problem in mind in studying intervals bounded by different stimuli was to determine whether the memory of intervals of medium length was dependent mainly upon the sense organ to which the interval was given, or to some adjustment, very probably motor, which took place after the stimulus reached the brain. The intervals from 2 to 6 sec. were found to be reproduced as accurately when the standard was bounded by lights as when bounded by sounds. For intervals above one second the eye seemed to be about as good a sense organ for perceiving time as the ear. As distinguished from this there was a decided break as soon as the interval was bounded by one light and one sound. While it is not impossible to suppose that this break was due to greater difficulty in perceiving the stimuli when coming to different organs in succession, such a view does not seem to me to be likely. We can turn our attention, at this length of interval, with comparative ease from a sound to a light, but if we attempt to make a movement first to a sound and then to a light we find the effort considerable. If, now, we were to suppose that the memory of the time interval depended upon the muscular adjustment which followed the stimuli, we would have a fitting explanation of the break which

occurs in remembering and reproducing an interval bounded by two unlike stimuli, as compared with the same interval bounded by like stimuli. This result, therefore, seems to have a bearing upon any theory of the time sense which places the emphasis on a muscular adjustment rather than upon the fading of the sensations or the perception of a monotone of bodily feeling during the interval. Münsterberg has developed the theory that we try to reproduce the same feeling of strain in judging time intervals. He pays especial attention to judging by respiratory rhythms for longer intervals.[1] Horwicz has suggested that movements of various parts of the body are used for time judgments.[2] Wundt emphasizes the limb movements.[3] Meumann found that the bounding stimuli might be changed in intensity without materially affecting the memory of intervals which were over half a second long. He concludes that for intervals longer than this the sensations are of comparatively small importance.[4] My experiments were conducted with intervals 1, 2, 3, 4 and 6 sec. long. They indicate that there is a difference in constant error between the light interval and the sound interval for 1 sec. but not for 2 sec. or above. Moreover, they show that even for 1 sec. there is a decided break between the interval bounded by like and by unlike stimuli.

Method of Experiment.

In my experiments I arranged as far as possible to avoid both practice and contrast effects. Five subjects were tested. Each of the intervals was reproduced twenty times in each of the forms of stimulus in which it was presented. The same interval was never given twice in succession nor was any regular progression followed. The order was determined by a chance arrangement which was not repeated until the subject had made a total of twenty-five reproductions, so that the reactor was always ignorant of what interval was to be presented next. To further avoid practice and fatigue, reproductions were made for only an hour at each sitting. I wish to lay stress on the advantage of the

[1] Beiträge, 1889, Heft 2, p. 1 ff.
[2] 'Psychologische Analysen auf physiologischer Grundlage,' Band II., Heft 3.
[3] 'Outlines of Psychol.,' Judd trans., p. 159.
[4] *Philos. Stud.*, XII., 129.

subject not knowing what interval he is to reproduce. From a test made with one subject in reproducing the same interval in succession, I found a correlation of .63 with a standard error of .03 between the way he reproduced the interval one time and the next. This shows a strong tendency for over- or under-estimation in one judgment to affect the next judgment of the same interval if that is immediately repeated. The effect of such a large correlation must be considerable in all investigations where the subject continuously judges the same interval. Under a chance arrangement of the intervals this effect of the previous reproduction should eliminate itself. I found no effect of practice between the earlier and later judgments under this varied arrangement. Not only were the intervals given in a chance order, but after five reproductions had taken place with each of the sound intervals, the series was changed to lights, then light-sound and sound-light intervals in a double fatigue order, so that all the different forms of the standard would have any advantage there might be from practice with the others. Sound intervals could thus be compared with any of the other series. By these methods I am confident that more correct information as to time judgments was obtained from averages with a small number of reproductions than could have been acquired with hundreds of reproductions otherwise.

For a sound stimulus I used the click of a telegraph sounder. For a light stimulus I used a streak of bright light, 50 by 4 cm., thrown by an electric lantern on a screen. A cardboard disk, 40 cm. in diameter, revolved in front of the lantern, allowing the light to pass through a radial slit in it, 2 mm. wide, when this came in line with a narrow slit in the focus of the lantern. The disk contained two such openings 180° apart. It was revolved by a heavy iron color wheel connected to a motor through a speed reducer. The error of rotation was exceedingly small and entirely negligible for the length of interval used. Attached to the center of the disk was a small wheel, 5 cm. in diameter and 1 cm. thick, arranged to make an electric contact every time the openings in the disk came in line with the light of the lantern to produce a flash. The contact was made through two light flat springs which rested continuously against the rim

of the small wheel. The current passed between the springs when they touched two brass contacts, connected by a wire and set into the circumference of the wheel 180° apart. This electric current passed through the sounder, while a switch in the circuit allowed the operator to bring in or shut out the sounder as he chose. Between the disk and the cloth on which the flashes were thrown was placed a screen containing a mechanical shutter covering an opening 15 cm. square. This screen cut off the light from the observer's screen until the shutter was opened by the operator moving a lever. The disk on the color wheel was revolved continuously. By means of the switch and shutter the operator could give either a sound or light stimulus to the observer every second or multiple of a second. The subject was placed in a room adjoining that containing the apparatus. The sounder was on a table beside him and the lights appeared on a cloth screen above the table in a window between the two rooms. Within a convenient time after receiving the interval the subject reproduced it by two pressures on a telegraph key. His reproduction was registered on a continuous-roll kymograph. The record was traced in ink by a glass capillary pen attached to an electric marker. The record line was paralleled by an ink line showing seconds. By allowing about 30 mm. space on the paper to each second, the reproductions could be measured with sufficient accuracy. The tables show that the average variability of the subjects in reproducing an interval of 1 sec. was about .2 sec. The average error of the producing and recording apparatus combined was less than .01 sec., which is well outside of the range that would disturb the results. As the subject could not see the apparatus, there was nothing to distract his attention from the time judgment unless it was the whir of the motor. This was very much reduced by mounting the motor on a thick layer of felt. Moreover, it was a constant sound which would not disturb the result for the comparison desired.

The results of the investigation for the five subjects tested are given in the table below. The average for each individual for each arrangement of stimuli is from twenty trials. The average of each group of five individuals is thus for 100 repro-

ductions. The averages are given with their average variability. The error of the individual averages may be determined in the usual way by dividing by the square root of the number of trials.

Subjects.	1 Sec.		2 Sec.		3 Sec.		4 Sec.		6 Sec.		
	Av.	Av. V.	Av.	Av. V.	Av.	Av. V.	Av.	Av. V.	Av.	Av. V.	
A	1.09	.12	2.07	.24	3.03	.29	3.91	.44	5.76	.50	Sounds.
B	.74	.13	2.23	.52	2.85	.73	4.47	.95	5.78	1.16	
C	1.01	.17	1.64	.28	2.27	.46	2.78	.45	3.66	.42	
D	1.07	.23	2.34	.55	3.18	.61	3.78	.54	5.41	.65	
E	1.12	.28	2.15	.33	2.85	.34	3.51	.58	4.59	.76	
	1.01	.10	*2.09*	.18	*2.84*	.26	*3.69*	.43	*5.04*	.73	
A	1.16	.11	1.90	.32	2.90	.29	3.74	.35	5.64	.56	Lights.
B	1.18	.23	2.21	.75	4.10	1.30	4.76	1.33	6.71	1.56	
C	1.00	.25	1.70	.35	2.21	.40	2.55	.49	3.68	.71	
D	1.63	.57	2.58	.52	3.09	.44	3.87	.83	5.04	.79	
E	1.79	.66	2.17	.47	2.94	.58	3.56	.57	4.50	.64	
	1.35	.29	*2.11*	.25	*3.05*	.44	*3.70*	.51	*5.11*	.85	
A	1.27	.17	2.83	.97	3.23	.73	4.46	1.09	6.12	1.00	Sound-Light.
B	2.25	.85	3.31	.93	4.51	1.21	4.99	1.28	6.74	1.18	
C	1.49	.50	2.05	.33	2.21	.45	3.11	.45	3.68	.90	
D	1.48	.37	3.03	.62	3.02	.52	3.74	.62	5.06	.65	
E	1.59	.49	2.47	.61	3.15	.45	3.89	.44	5.33	.59	
	1.62	.20	*2.74*	.38	*3.22*	.51	*4.04*	.55	*5.39*	.83	
A	1.28	.28	2.49	.50	3.36	1.07	5.28	1.02	7.11	1.35	Light-Sound.
B	1.68	.49	3.65	1.21	4.51	.88	5.84	.93	7.45	1.42	
C	1.47	.48	2.22	.47	2.73	.61	3.02	.44	4.10	.93	
D	1.73	.37	2.71	.58	3.12	.53	4.09	.63	5.25	.60	
E	2.13	.78	2.54	.48	3.18	.49	4.07	.52	5.63	.77	
	1.66	.23	*2.72*	.35	*3.38*	.43	*4.46*	.88	*5.91*	1.03	

An examination of the table will show that, with the exception of the 1 sec. interval, the average reproduction was almost the same for lights and sounds. A sharp break, however, occurs in the averages between the intervals bounded by like and by unlike stimuli. I have plotted this difference in the accompanying figure. The averages maintain a difference of almost half a second from each other through all the intervals. The reproduction of the intervals bounded by a light and sound is consistently lengthened. A study of the differences with their variability, in connection with a table of probabilities, will show that we can say the chances are nine out of ten that the 1 sec. interval will be lengthened at least .3 sec. when it is

bounded by unlike stimuli, over the length when bounded by like stimuli. There is a like chance that the 2 sec. interval will be lengthened at least .4 sec. On account of the greater error of the averages for longer intervals, we cannot say more than that the chances remain high, .90 or above, that the average reproduction of intervals bounded by unlike stimuli will be longer.

The averages indicate that the intervals of 4 and 6 sec. given by a sound followed by a light are somewhat less difficult

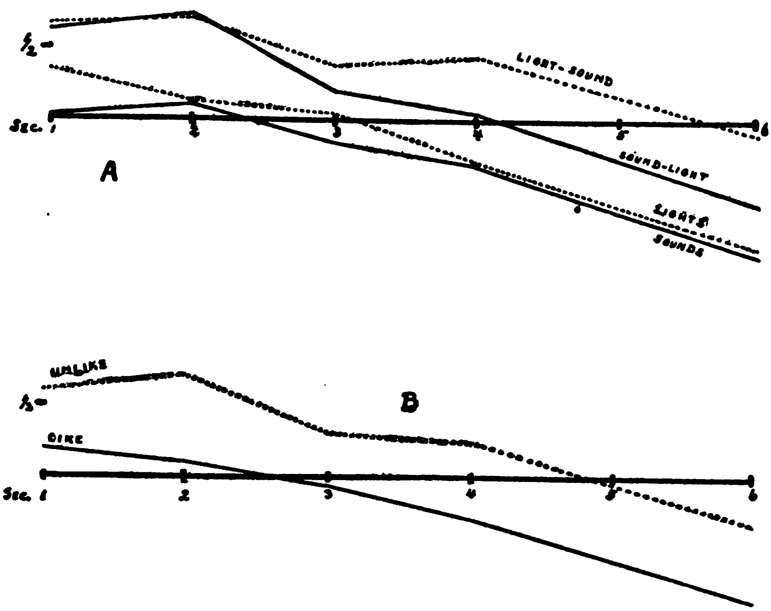

FIG. 6. (*A*) Averages plotted for the reproduction of intervals bounded in various ways. (*B*) Averages for intervals bounded by like stimuli, two sounds or two lights; compared with those bounded by unlike stimuli, sound-light or light-sound.

to follow than when given by a light followed by a sound. It is possible that this may have some bearing on the controversy as to which arrangement of these unlike stimuli is easiest to perceive. Exner found that a light-sound interval of .016 sec. could be distinguished, while a sound-light interval must have the stimuli separated by .06 sec.[1] This has been disputed by Bloch, Tracy and Hamlin. Whipple has recently reviewed

[1] Cited by Wundt, 'Physiol. Psychol.,' 4te Aufl., II., 392.

the subject and made extended experiments under careful conditions. He concludes that the sound-light arrangement for very short intervals is more easily perceived.[1]

Variability.

A study of the variability of the subject in reproducing the intervals in the different forms develops two interesting facts. (1) The subject is somewhat more variable in his work with light intervals than with sounds. This brings out a difference between adjustment to auditory and visual stimuli which was not apparent in the constant error for intervals above 1 sec. A table of averages of the individual average variabilities for sounds and lights is given below. The average error of each average is given after it.

	1 sec.		2 sec.		3 sec.		4 sec.		6 sec.	
	Av.	Av. E.	Av.	Av. E.	Av.	Av. E.	Av.	Av. E.	Av.	Av. E.
Sounds,	.19	.02	.38	.07	.49	.07	.59	.07	.70	.10
Lights,	.36	.10	.48	.06	.60	.14	.71	.09	.85	.14

(2) The increase in the variability with the larger intervals shows a remarkable tendency to follow the law suggested by Cattell and Fullerton.[2] This law supposes that the variability will increase in proportion to the square root of the magnitude dealt with, instead of an increase proportional to the magnitude as suggested by Weber. Nichols gives a tabular review of early investigations on time in which he shows Glass, Ejner, L. T. Stevens as holding that the increase in variability approximately follows Weber's law, while Mach, Vierordt, Kollert and Estel decide it does not hold.[3] Nichols[4] himself decides against Weber's law, as do Shaw and Wrinch[5] and H. C. Stevens more recently.[6] The following table gives the relation of variability for the different intervals as it would occur according to the suggestion of Cattell and Fullerton and as it was actually found for the averages of the individual variabilities in my

[1] 'On Nearly Simultaneous Clicks and Flashes,' *Amer. J. of Psychol.*, X., 280.

[2] 'On the Perception of Small Differences,' *University of Pennsylvania, Philos. Series*, No. 2, 1892, p. 23.

[3] *Amer. J. of Psychol.*, III., 519.

[4] *Op. cit.*, p. 529.

[5] *University of Toronto Stud.*, *Psychol. Series*, No. 2.

[6] *Amer. J. of Psychol.*, XIII., 23.

work. For convenience of comparison the relation is expressed in terms of the variability for 1 sec., taken as a standard.

Intervals in seconds,	1	2	3	4	6
Cattell-Fullerton Law,	1	1.4	1.7	2.0	2.45
Average for all forms,	1	1.4	1.6	1.8	2.3
Average for light intervals,	1	1.4	1.7	2.0	2.37
Average for sound intervals,	1	2.0	2.5	3.0	3.6

Indifference Points.

The question of indifference points in the estimation of time intervals has for years interested investigators. My own results in this connection have their main value in showing how a slight change in method may produce significant changes in the result. This is important in interpreting the meaning of any node of exact judgment that may be found. Estel contended that there is a normal interval of .75 sec. which, with its multiples, is most accurately judged.[1] Wundt suggested that this about agrees with the period of the leg swing in walking. Should this conception be retained, or should we hold that the node varies so much with the method of experimentation that we cannot believe it depends on a general interval most favorable for judgment? How far are we entitled to say that time estimates depend upon a normal interval habitual to the mind, when we find that different investigators have found different norms; that even where very similar methods have been used different indifference points have been demonstrated; that these are in one place for sound intervals, another place for lights, etc.; that they vary widely with different individuals and even with the same individual; that the same interval is at one time overestimated and at another time underestimated with a certainty that would not be overthrown by chance? Under such conditions, if they exist, we should be very careful about supposing time judgments to be dependent upon any specific bodily rhythm.

That the average reproduction of a time interval varies a significant amount under very similar conditions maintained by different investigators is shown by a comparison of Seashore's[2] results and my own. We both used intervals of one and two

[1] *Philos. Stud.*, II., 37.
[2] *University of Iowa Studies in Psychol.*, II., 74.

seconds, and judgments were made by the method of single reproduction with two taps on a key. His procedure seems to have been almost identical with mine for sound intervals except in two particulars. He gave his subjects the intervals for judgment in a 'double fatigue' order, while I gave them in a chance order to eliminate progression effects. He told his subjects not to use any aids in remembering the intervals, while I preferred to leave mine uninstructed on this point. In presenting the results for comparison I have averaged his records for men and women together, as they do not seem to show a significant difference for these intervals. I give the average constant error in the reproductions and the average error of these averages. Seashore tested 46 individuals and I only five, so that my averages are more inaccurate.

	Seashore.		Miner.			
Interval.	Av.	E.	Av.	E.	Difference.	Av. V.
1 second,	—.09	.005	+.01	.05	.1	.05
2 seconds,	—.112	.02	+.09	.09	.51	.09

For the 2 sec. interval the chances are thus even that there is a difference between these averages amounting to .51 ± .09 sec. The error of my average for the 1 sec. interval is comparatively large, but we can still say that there is a difference between Seashore's results and my own amounting to .1 ± .05 sec. with a probability of about one to one. Even when two observers use the single reproduction method in as nearly the same form as Seashore and I did, we see that the estimate of a 2 sec. interval shifts on an average about half a second. This of course means quite a change in the position of the indifference point. It is difficult to suppose that the judgment of time depends upon any absolute norm when so slight a change in conditions causes the estimate of an interval of 2 sec. to change a quarter of the magnitude judged.

My results also give an opportunity to compare the records of the same subject under two investigators. One of H. C. Stevens' subjects also acted for me. The records of this subject are different enough, so that we can say with a large probability that in this case a difference in method and not a difference in subjects produced new results. Stevens' method dif-

fered from my own in that he gave the same interval repeatedly to the subject and also in that he required the subject to make only one pressure on a key as soon as the same interval had passed after hearing the standard. In my work two pressures were made bounding the interval. Unfortunately we did not use exactly the same intervals. For the purpose of this comparison we may suppose that the change in time estimation was gradual between intervals of .9 and 1.3 sec., which Stevens used. By interpolating a value for 1 sec. to compare with my record and a value for 2.4 sec. between my 2 and 3 sec. intervals to compare with Stevens' record for 2.4 sec. we get some idea of how the results compare.[1] I have done this in the following table showing the record of the same subject under the two methods. The average constant error in estimation and the average errors of these averages are given:

Interval.	Av.	E.	Av.	E.	Difference.	Av. V.
1 second.	— .05	.005	+ .09	.025	.14	.025
2.4 "	— .117	.014	+ .05	.06	.167	.06
4 " *	+ .02	.07	— .09	.10	.11	.12
6 " *	+ .16	.42	— .24	.11	.40	.42

* Stevens' intervals were 3.75 sec. and 5.45 sec.

This table means that the chances are even that there is a difference between Stevens' results and mine for the same subject amounting to $.14 \pm .025$ sec. in the average estimate of an interval of 1 sec., and of $.167 \pm .06$ sec. for an interval of 2.4 sec. The errors are too great in the averages for the longer intervals for any conclusion to be drawn. Stevens states that he found that the indifference point was lowered from the average position of .72 sec. to .40 sec. when he changed the method of reproduction to one pressure on the key instead of two. It will be noticed that the difference between Stevens' results and my own for either 1 or 2.4 sec. intervals is sufficient to change the sign of the estimate as shown in the averages. Method thus makes the same subject change from over- to underestimating the same interval. This change in method, moreover, is quite slight compared with the change between the method of average error determined by reproduction and the method of judging the least perceptible difference when listening to sound intervals, which was used by early investigators.

[1] *Amer. J. of Psychol.*, XIII., 20.

My investigation of the multiple reproduction of an interval of 1 sec., which I shall present later, also shows a difference from the result of similar work done by Nichols and L. T. Stevens. My results with 145 subjects show that this interval, when reproduced continuously for about a minute, was underestimated on the average about .08 sec. The average error of this reduction is less than .01 sec. Nichols, on the other hand, with 15 subjects found that an interval of 1.25 sec. reproduced for about two minutes was overestimated on the average .05 sec.[1] From his tables I have calculated the average error of this overestimation to be .015 sec. Stevens found intervals between .87 sec. and 1.5 sec. overestimated, when reproduced for about one minute continuously.[2] As he gives no variations for his averages, comparison would be uncertain. It is not likely that the difference between Nichols' results and mine is due to the fact that his subjects continued their reproductions for two minutes, while mine only reproduced continuously for about one minute. I found that forty of my subjects who continued the reproduction for two and a half minutes showed still greater underestimation. This disparity in results seems to be another instance where conditions surrounding the experiment make a significant difference in time estimation under very similar methods. It seems to be further evidence that overestimation and underestimation are not closely dependent upon a comparison with a norm carried in the mind.

A study of the different investigations of time intervals that have heretofore been conducted brings out even more forcibly the danger in supposing there is quite a definite indifference point in time estimation. It is not necessary to go extensively into the literature of the subject. Nichols has done that for all except one or two of the recent investigations.[3] The table which he gives shows how various have been the indifference points selected, as well as the striking differences in the estimation of the same interval under different methods and observers.[4]

[1] *Amer. J. of Psychol.*, IV., 80.

[2] *Mind*, XI., 395.

[3] *Amer. J. of Psychol.*, III., 503-529. See also Wundt, 'Physiol. Psychol.,' II., 408; James, 'Principles of Psychol.' I., 616.

[4] *Amer. J. of Psychol.*, III., 528.

A few instances taken from his table bring out the point. Höring, using the method of right and wrong cases and judging without reproduction, placed the average indifference point for ten subjects between .365 and .454 sec.[1] Vierordt, also using the method of right and wrong cases, but having the subject reproduce the interval by two taps, found that for himself the indifference point was between 3 and 3.5 sec. and for two other subjects about 1.5 sec.[2] Kollert, using the method of least perceptible difference in judging the intervals given by two metronomes, assigned .755 as the indifference point, averaged from seven subjects.[3] This last indifference point has been somewhat corroborated by other observers. Mehner made a series of trials on himself, following substantially Kollert's method, and found a node at about .7 sec.[4] Glass, testing only himself, by the method of average error with a single reproduction each time after the standard, found one node at .7 sec.[5] L. T. Stevens, using multiple reproduction, also found a node there on the average of seven subjects.[6] Recently H. C. Stevens, with a single reproduction of the interval, places the indifference point about the same place.[7] The evidence at first seems to be quite convincing as to a node between .7 and .8 sec. It is to be remembered, however, that Mehner and Glass experimented only on themselves, and their conclusions are not sufficient for generalization. Nichols believes that Kollert's results are not reliable, since he rejected 42 cases as anomalies out of 175 trials.[8] Strong evidence as to the uncertainty of the process of time judgment seems to be furnished by the absolute contradiction among the above investigators as to the direction of the constant error immediately above and below this node of .7 sec., even when they agree as to the node. Kollert finds the intervals above (.755 to 1.836 sec.) underestimated. Mehner

[1] 'Versuche über das Unterscheidungsvermögen des Hörsinnes für die Zeitgrössen,' 1868, cited by Nichols.

[2] 'Der Zeitsinn,' 1868, cited by Nichols.

[3] *Philos. Stud.*, I., 88.

[4] *Philos. Stud.*, II., 546.

[5] *Philos. Stud.*, IV., 423.

[6] *Mind*, XI., 393.

[7] *Amer. J. of Psychol.*, XIII., 1.

[8] *Amer. J. of Psychol.*, III., 509.

finds the same for intervals from .71 to 5 sec. H. C. Stevens shows intervals from .72 to 2.4 sec. underestimated. Glass finds intervals underestimated in one set of experiments from .7 to 15 sec. and in a second set finds overestimation from .7 to 1.8 sec. L. T. Stevens and Nichols find overestimation immediately above .7 second. Nichols' suggestion that multiple reproduction will account for this difference will hardly hold, since Glass, using a single reproduction, found the results one time one way and the next time opposite, and my results with multiple reproduction show a decided underestimation of the 1 sec. interval.

In conclusion, it may be said that there is some evidence of an indifference point at about .7 sec. This evidence, however, seems inconclusive and is rendered less important by the flat contradiction as to the direction of the constant error above and below the node. At most it seems necessary to say that the node will be at that point only under a definite method of experiment. It seems probable that there are numerous indifference points, depending upon the conditions for the time judgment. These conditions are quite as important as any physiological norm in determining whether a certain interval will be under- or overestimated.

2. Multiple Reproduction of an Interval.

The problems of time have heretofore been investigated with either one subject or a few individuals. The material gathered in connection with the psychological tests at Columbia University gave me the opportunity to study a group of 145 students who had been tested for the multiple reproduction of an interval of 1 sec. With this large group it was possible to eliminate the individual differences and determine with accuracy what the effect is of a continuous duplication of the same interval for a period of forty seconds. The result has a bearing upon any study of time in which the method of multiple reproduction is compared with single reproductions. It is demonstrated with practical certainty that the speed of reproduction increases during a half minute of continuous repetition of the interval. This indicates that the averages presented for mul-

tiple reproduction, at least for a one-second interval, will be smaller than for a single reproduction. Allowance must therefore be made for this difference in any comparison of work on time.

The experiment consisted in having the subject tap on a telegraph key ten times in conjunction with the click of a telegraph sounder which was in the circuit of a seconds pendulum. The sounder was then shut off by a switch and the subject tried to reproduce the same interval of one second fifty times without stopping. The recording instrument used was especially devised at Columbia for this work and has recently been improved (see plate at the end of the thesis). In its present form it utilizes the narrow carbon ribbon of a typewriter and the paper tape of a telegraph ticker for making records with electric pens. The instrument is a clockwork kymograph with a continuous roll of paper tape. The new feature is the band of carbon ribbon (*R*), about a centimeter wide, which runs just above the paper. The record is made by the metal points of electric markers striking the paper against the carbon ribbon. A surface against which this marking takes place is provided by a rubber-covered peg (*A*). Two electric markers give the time line and the record line of the subject's reproduction. The time line is marked by arrow points showing the direction the tape is moving. This is done by shaping the metal head of the electric marker. The apparatus is enclosed in a dust-proof box with a sliding glass side. The kymograph is started by means of an electric magnet (*M*) that raises a rod which rests on the fan governor to stop the instrument. The current through the magnet may be made or broken by a switch in a room where the subject is. This is a decided advantage, as it allows the recording instrument to be placed out of hearing and yet started and stopped by the operator in the room with the subject.[1]

In the following table is given the average length of the one-second interval as reproduced during each of the four succeed-

[1] I wish to acknowledge my indebtedness to Prof. James McKeen Cattell for perfecting this recording instrument. The novel application of a carbon-ribbon to this use was made at his suggestion. It also gives me pleasure to credit Mr. E. Horstmann, the instrument maker at Columbia, with building the special pieces of apparatus used in this research and making valuable suggestions as to their mechanical construction.

ing periods of ten seconds. The average error of each average is also given. The table is averaged from 145 individuals. The variability may be found, if desired, by multiplying the error by the square root of the number of cases.

	1–10 sec.	11–20 sec.	21–30 sec.	31–40 sec.
Average,	.96	.93	.91	.88
Error of average,	.005	.006	.006	.006

The table not only shows that the interval of one second was underestimated throughout the reproductions, but that there was also a rather constant shortening of the period. A comparison of the length of the interval as reproduced during the first ten seconds and as reproduced during the last ten seconds shows that it was shortened .08 sec., plus or minus an average variability of .008 sec. In other words, the chances are even that this interval will be shortened between .07 and .08 sec. during forty seconds' reproduction. This shortening we might suppose is due to the hastening of a process as it becomes more automatic. On the other hand it would probably be a mistake to suppose that as the movement becomes more automatic the feeling of effort during each interval grows less. Mentally the subjects try to keep the feeling of effort alike for each repetition of the interval. Weariness with their task, however, makes a shorter muscular strain seem to amount to the same effort as the longer strain did at the beginning. The above suggestions would be in agreement with an explanation which based the sense of time on kinæsthetic sensations. They are doubtless not the only theory on which the phenomena may be explained, but are offered as harmonizing the facts with the general theme of the paper.

With forty-two subjects a longer series of reproductions was made, extending for two and a half minutes. For this entire period the tendency to shorten the interval is apparent. The shortening is not progressive throughout, nor is any periodic lengthening and shortening brought out by the averages. If there is such a rhythmic fluctuation it is covered up in averaging. An examination of the individual records does not seem to indicate any periodic rise and fall. The table below gives the average length of the 1 sec. interval as reproduced for each

succeeding ten seconds. The average error for each average is given below it.

Period.	1–10	11–20	21–30	31–40	41–50	51–60	61–70	71–80	81–90	91–100	101–110	111–120	121–130	131–140	141–150.
Average,	.90	.89	.86	.83	.85	.82	.83	.89	.83	.83	.83	.84	.83	.84	.83
Error of Av.,	.011	.012	.012	.012	.012	.014	.014	.014	.015	.016	.014	.016	.015	.016	.016

I have already noted that these results are in conflict with the investigations of L. T. Stevens[1] and Nichols,[2] who experimented with multiple reproductions of intervals. They both state that intervals over .7 sec. are overestimated. Stevens tested for intervals as large as 1.5 sec. and Nichols as large as 1.75. We cannot adequately compare Stevens' result as there is no opportunity from his figures to calculate the variability. Nichols' figures would indicate that the chances are even that the overestimation of an interval of 1.25 sec. will be between .03 and .07 sec. This would give a large probability for some overestimation. My own results for an interval of 1 sec. indicate an underestimation considerably greater in amount. With the above series of 42 subjects the average constant error was —.15 sec. with an average error of less than .015 sec. The probability of some underestimation rises here virtually to a certainty. The difference can hardly be due to the fact that Nichols used 1.25 sec. and I used 1 sec. as the standard. His results indicated that the overestimation began at .7 sec. and increased. His subjects reproduced for two minutes, mine for two and a half. I have suggested that the contradiction is best explained by supposing that any slight change in the conditions surrounding a time experiment will make a decided difference in the attitude of the subject.

3. Correlation of Time Judgments and Reaction-Time.

Does a short time interval seem longer to the quick person than to the slow? The question was suggested by Seashore, who inclined to an affirmative answer, although he was unable

[1] *Mind*, XI., 393.
[2] *Amer. J. of Psychol.*, IV., 80.

statistically to measure the relation.[1] By using the Columbia tests it has been possible for me to determine from a group of 140 subjects the exact correlation between reaction time and the reproduction of an interval of one second. In making the calculation the Pearson formula was used.[2] The coefficient of correlation is expressed under this formula as follows:

$$r = \sqrt{\frac{\Sigma xy}{n\sigma_1\sigma_2}}$$

The coefficient is 'r.' The variation of any particular case from the average of one of the correlated functions is 'x,' and its variation from the average of the other function is 'y'; 'Σxy' indicates the sum of these 'xy' products. In the denominator of the fraction 'n' stands for the number of cases, and the sigmas are the mean square variability of the averages of the two functions correlated.

In my calculation I used the subject's reaction time to sound, determined from an average of five trials. This was correlated with the average time in which the subject reproduced an interval of one second when making continuous reproductions for a period of 40 sec. The method of multiple reproduction which I used has been heretofore described. The average reaction time for the group of 140 subjects was .16 sec., with a mean square variation of .02 sec. The average length of the 1 sec. interval as reproduced by the group of subjects was .9 sec., with a mean square variation of .09 sec. The coefficient of correlation was found to be $-.55 \pm .046$. The error of the coefficient is determined by the following formula, the characters having the same significance as in the formula above:

$$\text{Error of } r = \frac{1 - r^2}{\sqrt{n(1 + r^2)}}.$$

The coefficient $-.55$ may be regarded as a high degree of correlation, it being almost as large as the intensity of correlation found by Pearson for physical characteristics. The highest coefficient which Wissler found between reaction time and the

[1] *University of Iowa Stud. in Psychol.*, II., 80.

[2] 'Grammar of Science,' 2d ed., 1900, p. 400.

functions with which he tested its correlation, viz., movement time, association time, marking out *A*'s and naming colors, was .15.[1] The fact that the coefficient is minus shows that those who have greater than the average reaction time (those who are slow) will give below the average length to the interval (make it shorter). If shortening an interval when reproducing it means that it appears shorter, then we must suppose that 1 sec. seems shorter to a slow person than it does to a quick person. As Seashore says: "Time that is associated with his own quick actions seems shorter to the slow person than to the quick person." Curiously enough Seashore seems to turn the correlation about in his subsequent discussion, for he supposes that the slow person overestimates the interval more instead of underestimates it, as my records indicate. If the slow person really overestimated the interval more, we must suppose that it seemed longer to him than to the average person, which would be just the opposite of Seashore's conclusion quoted above. His statement is as follows:

"A comparison of Tables XIII. and XVI. shows that the observers who overestimate the shortest intervals most, tend to have poor motor ability and reaction-time. * * * It is to be regretted that I cannot give a more adequate account of the observations upon this very important point. However, the gauging of the smallest interval by the measured voluntary motor ability, and the information elicited through questions in the extra trials, convince me that this overestimation of the short interval is not due to inability to act quickly enough, but it is a normal illusion in the perception of the standard interval. The evidences warrant the conclusion that the time that is associated with his own quick actions seems shorter to the slow person than to the quick person. Does the slow boy realize how long an interval elapses before he begins to reply to the teacher's question? And, when he has once started, do the intervals between his words seem as long to him as they seem to the quick boy at his side? The lagging of a slow person in all rushing activities does not seem as great to him as to the quick person. Persons who are habitually too slow in their quickest actions have established erroneous associations between standards of time and the time of their own actions."[2]

If the latter part of this statement is true, it is apparent that the slow person must underestimate more than the average. Whether the slow boy would show this same tendency to shorten the interval if he merely judged the interval without reproduc-

[1] 'Correlation of Mental and Physical Tests,' *Monog. Sup. Psychol. Rev.*, 1901, III., 61.

[2] *Op. cit.*, p. 80.

ing it, we cannot say. We should expect the same tendency to hold and are probably justified in supposing that the interval is actually perceived differently by the slow boy. We are certain that there is a correlation as to the reproduction of the interval. In this correlation we have again an indication that time estimation is dependent in some intimate way on the muscular reaction of the individual. This is theoretically the most important feature of this correlation.

This tendency of time estimation and reaction-time to go together has a further bearing on all investigations of time. It provides a definite grouping in which subjects who underestimate or overestimate intervals may be classified. We know that there are specific classes of people who are characteristically different in this function. If, then, an investigator has unintentionally selected a group of subjects with quick reaction-time, their judgments on short intervals will show a plus tendency from the average time estimation. This may give us a hint toward explaining the differences that have arisen between the results of different investigators. It would be especially important where results are published for only one observer. It shows the desirability of having groups large enough for the effect of such a selected class to be eliminated.

In everyday life the fact that an interval appears longer to a quick person has numerous applications, as Seashore suggests. Speakers who adapt themselves to slow hearers may find themselves tedious to the quick members of their audience. The beating of a baton of an orchestra leader may be more confusing to the slow person because the time seems more rapid. On the other hand the rapid person, we should expect, would really play more slowly on his instrument because the time of the leader seems slower to him. He lengthens the interval in reproducing it. The same principle would appear in connection with the operation of any machinery where rhythmical movements were required. Paradoxical as it may appear, the slow person seems actually to be faster if left to keep up a regular movement. Reaction-time might even become a test for selecting slow employees to make fast rhythmic movements or quick employees when it became necessary that short times should seem longer and less confusing.

PART IV. APPLIED RHYTHMS.

The intimate relation between rhythm and work opens numerous inviting avenues for experiment. The investigation of memory by Ebbinghaus at once suggested a connection with rhythm.[1] Subjects were found to group nonsense syllables as they learned them. This factor was studied further by Müller and Schumann, who observed a loss of almost half when rhythm was eliminated from the memorizing process.[2] That rhythmic grouping may increase the span of consciousness was shown by an ability to compare accurately large groups of successive stimuli, when these were broken up into smaller rhythmic units.[3] Scripture has called attention to the relation between natural rhythms and periods of work. When the two coincide we get the 'most action with the least fatigue.'[4] This is recognized in the 'rout step' of troops on long marches, when each man chooses his own pace.[5] It has been a favorite pedagogical method to utilize rhythms in memorizing the multiplication table, the names of the presidents, etc. An interesting collection of these rhythmic memory helps has been made by Sears.[6] Féré has recently published an excellent experimental study measuring the amount of work done on the ergograph when pulling in different rhythms. Particularly striking is the rapid fatigue following an increase in work when using certain rhythms.[7] The most extended laboratory study of 'Rhythmus und Arbeit' is that of Smith.[8] Besides comparing the effect of different rhythms

[1] 'Ueber das Gedächtniss,' Leipzig, 1885.

[2] 'Experimentelle Beiträge zur Untersuchung des Gedächtnisses,' Leipzig, 1893.

[3] James, 'Principles of Psychol.,' I., 407, reference to work of Wundt, Dietze, Bechterew, etc.

[4] 'New Psychol.,' p. 181.

[5] *Studies from the Yale Psychol. Lab.*, VII., 107.

[6] *Amer. Jour. of Psychol.*, XIII., 28 ff.

[7] *L'Année Psychol.*, 1902, VIII., 49 ff.

[8] *Philos. Stud.*, XVI., 71 ff.

in oral and visual memorizing, she opened up several other fields for the application of rhythms to work. Her experiments with writing in regulated rhythms and those with judging weights lifted in rhythms are explorations in new territory where trial alone can tell us whether we shall here discover new ways to increase ability.[1] If the ultimate aim of scientific inquiry is to increase human efficiency, considerable time must be occupied in psychological exploring. Should a small fraction be added to the productivity of individuals in even one activity the reward will be great. The general problem, how to increase efficiency, carries with it a somewhat simpler problem, how to improve most rapidly. We are constantly testing various methods of training and working out by the slow rule of thumb the method best adapted to speedy improvement. Rhythm appears as one of the most promising guides pointing into the future. Our general knowledge of this phenomenon affirms its economical value in all activity. When seeking for increased ability or more rapid improvement we may hope for success if the work in hand can be adapted to a rhythm. May a person not learn to use a typewriter more quickly if the keys are operated in a rhythm? May rhythm not be utilized in learning to spell long words? Suppose the child should say, 'M-i′ double s-i′ double s-i′ double p-i′,' would he not more quickly get the correct spelling of the word? The use of rhythm might save us many a stumble in articulation. We know that rhythm is necessary to pronounce many words and has worked itself into difficult combinations of syllables through primary and secondary accents, *e. g.*, pres′-ti-dig′-i-ta′-tor. Pleasing prose sentences probably maintain a rhythmic balance. We have only made a beginning in the investigation of the relations of rhythm to poetical thought. Numerous other possibilities for the adaptation of rhythm to mental activities will suggest themselves.

The two series of experiments which I shall report may be considered as explorations in this wide field of applied rhythms. I have varied the usual method by trying to find the effect of

[1] As this thesis is going to press, another paper on 'Arbeit und Rhythmus,' by Von Dobri Awramoff, *Philos. Stud*, XVIII., 515–563, has just been published. It treats of the effect of rhythm on the ergograph pull, reaction-time and writing.

an independent rhythm on mental activity, instead of having the subjects work in a prescribed rhythm as other investigators have done. In the first series of experiments I tried the effect of requiring subjects to supply words in the blanks left in a poem, while they were themselves continuously beating a rhythm with the fingers of the hand not used in writing. This gave the effect of an independent motor rhythm on a complex mental activity. In the second series I tried the effect of the rhythmic beat of the metronome, which was sounded rapidly or slowly beside a person, while he was distributing a pack of playing cards in the four suits. This showed the change produced by an objective rhythm on a rather simple mental activity in the nature of a continuous choice reaction. The rhythmic accompaniment in each experiment was thus in no way connected with or adapted to the work performed. Both series were conducted with groups of over a hundred subjects, so that I was able to work out definite correlations which proved to be the most valuable results of the investigation. The effect of the independent stimuli on the average amount of work done was slight, but the correlations led to the interesting discovery that rhythms affected the naturally slow person and the naturally quick in a decidedly different manner. The slow person, under rhythmic stimulation, tends to improve in efficiency, while the quick person tends to lose very decidedly. I shall discuss the results further after describing the experiments.

1. Effect of a Rhythmic Movement on a Complex Mental Activity.

This series of experiments was suggested by the common observation that many people make slight rhythmical movements while conversing, lecturing, or engaging in mental work of various kinds. The movements are usually with the fingers, such as tapping on a table, handling a watch charm, etc. The story is related of a German professor who was obliged to dismiss his class when he found that some one had cut off the coat button he was accustomed to fumble while lecturing. May we not get some clue to the effect of these movements on mental activity? If a satisfactory test for intellectual work could

be found, it would be a simple matter to compare the records of subjects made normally with those made while the same subjects kept up a constant tapping with their fingers. The test which I decided upon consisted in requiring the subjects to fill words in blanks which were left in the verses of Stevenson's poem 'The Dumb Soldier.' It was necessary that each subject should make the test with and without the motor accompaniment, so I divided the poem into two parts. These were mimeographed on separate sheets of paper from a typewritten copy. Each part started with the first stanza of the poem, given completely, so that the meaning of the piece might be easily grasped. Five blanks were left in each of the other stanzas. The two parts, except the first stanza, are printed in parallel below.

When the grass was closely mown,
Walking on the lawn alone,
In the turf a hole I found
And hid a soldier underground.

Spring and daisies —— apace;
Grasses —— my hiding place;
Grasses —— like a —— sea,
O'er the lawn up to my ——.

—— the grass is —— like grain,
When the scythe is stoned again,
—— the —— is shaven clear,
Then my hole —— reappear.

He has lived, a little thing,
In the ——woods of ——;
Done, if —— tell me true,
Just as —— should like to ——.

In the silence he has ——
Talking bee ——ladybird,
And the butterfly has ——
O'er —— as he —— alone.

Under —— alone he lies,
Looking up with leaden ——.
Scarlet —— and pointed ——,
To the stars —— to the sun.

—— shall find him, never fear,
I —— find —— grenadier;
But for all that's gone and come,
I shall ——my soldier ——.

He has seen the starry ——
And the —— of the flowers
—— the fairy things —— pass
In —— forests of the grass.

Not a word —— he disclose,
Not a word of all he ——.
I must lay —— on the ——,
And make up the —— myself.

The subjects for the experiment were students in the high school at Berlin, Wisconsin, and were quite evenly distributed through the four grades. Records of 102 subjects were obtained. The tests were given to a class of from 15 to 20 at a time. The directions were read to each group to be sure that they were always given in the same way. I conducted all the tests myself, thus

further insuring uniformity. The subjects were carefully cautioned to begin at the first and not skip any blanks, to place only one word in each blank, and to be sure that the word supplied made sense in the line of the poem, but to pay no attention whether it made sense with the preceding or following lines. At a given signal they began and wrote as rapidly as possible until they were told to stop (85 seconds). When the work was to be done with the rhythmical accompaniment the subjects were told to tap gently on their laps or papers with the fingers of their left hands. I kept watch and warned them to keep up the tapping whenever I found that any one stopped. In half of the groups the test was made first with the rhythmic stimulus and afterward without it. In the other half the normal test was made first and the stimulus afterward. To further eliminate any effect of practice or difference in difficulty between the two parts of the poem each alternate person was given the first part of the poem and his neighbors the other part. By this means the two parts of the poem were used equally in all the forms under which the test was given, so that any difference between them was eliminated in the averages. The test proved to be very satisfactory. The difference in difficulty between the two parts of the poem was on the average only .5 of a word. Only one subject succeeded in filling all the blanks in the time allowed. After finishing the tests each subject was asked to write on his paper whether he thought the rhythmic accompaniment had made the work harder or easier, or had no effect. There were 56 who thought it was harder with the movement, 19 thought it was easier and 27 thought there was no difference. Of these 102 judgments it is interesting to note that only 58 were right, as judged by the objective results. This is partly accounted for by so many stating no effect from the movement.

The results of the tests were as follows. Normally the subjects wrote on an average 4.7 words. The mean square, or standard, variability of this average was 3.3. The standard error, found by dividing by the square root of the number of cases, was .3 word. On the whole the number of words written under stimulus was less. The average loss was 1.1 word, with

a standard variability of 3.2 and error of .3. That is to say, the chances are .68 that the average loss under rhythmic movement would be $1.1 \pm .3$ word.

Interesting as this result may be, it is overshadowed by the discovery that the loss is far from uniform for different kinds of workers. The tendency is for the principal loss to fall upon the subjects who normally made an excellent record. Those who were naturally slow in this kind of work seemed to gain slightly under the motor stimulus. Grouping all the individuals who normally did better than the average, I find that in the test with tapping they lost on an average 3 words, plus or minus a standard error of .4 word. On the other hand those who normally wrote less than the average of 4.7 words, wrote an average of $.9 \pm .3$ word more when using the motor accompaniment.

To place this result in a more general form I have calculated the correlation between the records of the same subject as to his variation from the normal average and his variation under the rhythmic accompaniment from the average loss of 1.1 word. Using the Pearson formula the correlation was found to be .75, with a standard error of .04. This means that there is an exceedingly strong tendency for the subjects above the average in the normal test also to be above the average in the amount of loss under the stimulus. On the other hand those below the average normally lose less than the average, even tending to show an average gain with the motor accompaniment.

By using the coefficient of regression we may calculate what will be the average of any array in the correlated trait. The coefficient of regression ($\mathcal{Q}$) is determined as follows:[1]

$$\mathcal{Q} = r\frac{\sigma_1}{\sigma_2}.$$

In this formula r is the coefficient of correlation, σ_1 the mean square variation of one of the correlated traits from its average, and σ_2 the mean square variation of the other trait, in which it is sought to find the regression toward the average. Since in the above correlation the two sigmas are 3, $\mathcal{Q}$ will

[1] Pearson, 'Mathematical Contributions to the Theory of Evolution,' *Philos. Trans. of the Royal Society*, London, 1896, CLXXXVII., 268.

equal r and be .75. We may then say that those who write, for example, three words more than the average will tend to show $3 \times .75$, or two words more than the average loss under the rhythmic stimulus. Since the average loss was one word, they will tend to lose 3 words. It will be noted that this agrees with the empirical results stated above. The advantage of the coefficients of correlation and regression is that they make it possible to determine by a simple calculation the position of any selected part of the group of individuals in the correlated trait. Moreover, the coefficients are the most accurate methods of description.

2. Effect of an Objective Rhythm on Choice.

In the following experiments I used for a continuous choice reaction the distribution of a pack of playing cards in the four suits: spades, clubs, hearts and diamonds. After each distribution the pack was shuffled carefully three times so as to get a chance arrangement as nearly as possible. The experiments were to test the effect of the rhythmic beating of a metronome upon the work done by the subjects in distributing the cards. A series of records was taken when the subjects were working normally, another when the metronome was beating at the rate of 40 per minute with the bell sounding on every alternate beat, and a third series with the metronome beating 200 per minute with the bell on every alternate beat. One of these rates was slower and the other considerably faster than the speed at which the subjects distributed the cards. The metronome was placed on a sounding box to make it louder. In each test the cards were to be distributed as rapidly as possible for 25 sec. The subjects, 119 in all, were students in Teachers College, Columbia University. They were divided into groups of ten. The members of a group arranged themselves conveniently around a large table. They were directed when they distributed the cards to arrange the four suit packs in the form of a square; the two packs farther away to be the red suits and the two nearer packs the black suits. This avoided any variation there might be from different arrangements of the suits when distributing. Three tests were made on each subject without

the metronome, three with the metronome beating 40 and three with it beating 200. Instead of using the average of the three tests I found it best to use the middle or median record. A single record was frequently seriously disturbed, but by using the median record I believe a fair test was obtained for my purpose of correlating the work of the subjects under the three methods. The conditions were varied after each distribution of the cards, so that the practice effect would be equally distributed through the three forms. For example, if the subjects began without the metronome, the next time they distributed with the metronome 40, then with it 200, then without, etc. Each group of ten subjects began differently so as to further eliminate practice effects.

Taking up first the results for the continuous choice reaction made normally with that made when the metronome was beating 200 per minute, we find that without the metronome the subjects distributed in 25 sec. an average of 40 cards, with a mean square variation of 6 and a standard error of .6. Under the stimulus of the 200-rhythm they gained on the average .7 card, with a standard variation of 3 and an error of .3. I have calculated the correlation between the records of each individual compared with the normal average and with the average gain. The coefficient of correlation was —.32, with a standard error of .08. This means that those who normally distributed more than the average number of cards tended to fall below the average gain when they were under stimulus. We may calculate the coefficient of regression ($\mathcal{Q}$) under the formula given previously as follows:

$$\mathcal{Q} = -.32 \times \frac{6}{3} = -.6.$$

From this we can say that those who normally distributed 5 cards, for example, more than the average, would tend to be $5 \times -.6 = -3$ from the average gain under the stimulus. Since the average gain was .7, they would be in the position $-3 + .7 = -2.3$, or they would lose on the average 2.3 cards. On the other hand those who normally were slow, say 5 cards below the average, would tend to gain on the average $3 + .7 =$

3.7 cards under the rhythmic stimulus. We see, therefore, that the opposite effect is considerable on the two classes of individuals below and above the average in this sort of mental work.

The results with the rhythm of 40 beats per minute were as follows. The average gain under stimulus was practically nil. Accurately, it amounted to .3 word, with a standard variation of 3.5 and a standard error of .3. The opposite effect on the two classes of individuals, the quick and the slow, was again quite noticeable. The correlation, calculated as before, was —.39, with a standard error of .07. This would give a coefficient of regression of about .8. Using the same example as above, we can say that those who normally distributed 5 cards more than the average would tend under the 40-rhythm to lose on the average 3.7 cards; while those 5 cards slow compared with the average will tend to gain on the average 4.3 cards.

It might be supposed that familiarity with the use of playing cards would seriously disturb the results in an experiment like the above. To determine if this was likely I asked each person to grade himself as *A*, *B*, *C* or *D* as to his familiarity with cards. '*A*' meant that he was very familiar and '*D*' that he had almost never handled cards. Out of the 119 subjects only 7 classed themselves *D* and 12 classed themselves *A*, so that the mass of the subjects were in the middle grades. I have examined the '*xy*' products of these 19 extreme subjects (see formula for the coefficient of correlation under the correlation of time judgments and reaction-time), and I find that in both the correlations calculated the products of these individuals are very near the average. The amount of correlation shown in no way rests upon the records of these individuals. I think, therefore, that we may fairly hold that familiarity with cards did not disturb the result arrived at. Another factor that should be considered in the above test was that ten of the subjects, according to their median records, distributed all the cards in at least one form of the test. Two of these distributed all the cards under each form of the test. Of course we cannot tell how many more cards these people might have distributed, but the cases are so few and the difference would have been so small that the error is negligible.

To see what effect the subjects thought the metronone had upon their work I asked all of them, after they had finished the tests, to state under which conditions on the whole they thought they made the best record. As the subjects had not kept their own records they could answer only from such impressions as they had gained while making the tests. The answers were as follows: Best without metronome, 34; with metronome slow, 28; with metronome fast, 25; no difference, 32. In each of these cases about half of the answers agreed with the median records of the subjects. Judging by these records the number who actually did best under each form of the experiment was as follows: Without the metronome, 30; with slow metronome, 39; with fast metronome, 40; two out of three forms alike, 10.

3. Summary.

A rather important principle as to the relation between mental work and an independent rhythmic stimulus seems to be pointed out by the above experiments. On the whole the slow person is quite likely to profit from an independent rhythmical stimulus, while the quick person is very much disturbed. This seems to hold for the simpler mental activity of distributing cards as well as for the complex exercise of supplying words for blanks left in a poem. The conclusion is supported by a correlation of .75 in the poem test between normal work and the effect produced by a rhythmic motor accompaniment, also by correlations of — .32 and — .39 in the experiment with choice between the records in the work done normally and the gain made under an objective sound rhythm. A hint as to the possible explanation of this result is suggested by the difference in attentive attitude of slow and quick subjects. A person who is above medium in any mental activity will attend to it more keenly; will, as it were, be keyed to a higher pitch, or be in a state of more sensitive equilibrium. Any independent, secondary stimulus would thus serve seriously to disturb such a subject. To speak figuratively, he would have the edge taken off his attention. On the contrary the subject who does mental work indifferently will be excited by the rhythmic accompaniment and spurred to greater effort. The independent stimulus, which is

ordinarily supposed to be distracting, seems in the latter case to favor more rapid work. The discovery of these two classes of individuals, who show contrary tendencies while working under independent rhythmic stimulation, suggests that we are always in danger when we suppose that a method of increasing efficiency which is valuable for one person will be valuable for another. While it may be of advantage for the keenly attentive person to work in a quiet room and to inhibit all extraneous movements, these experiments indicate that for the slow person independent sounds or accompanying movements might even be an advantageous means of quickening mental activity. Perhaps we are making a serious mistake to insist that scholars keep quiet while working in school. It may be that what we call 'nervous movements' in their case drain off superfluous energy and allow their brains to work more actively. It is at least strongly suggested by these experiments that in the quick and the slow individuals we have two groups who require decidedly different treatment if they are to reach their highest efficiency.

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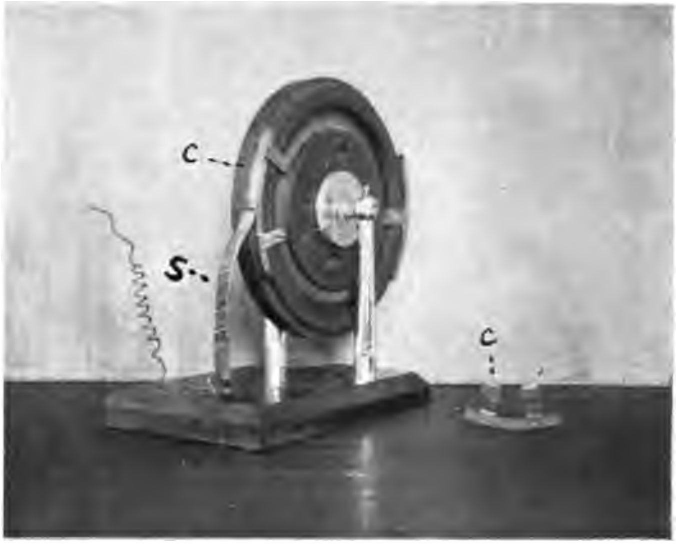

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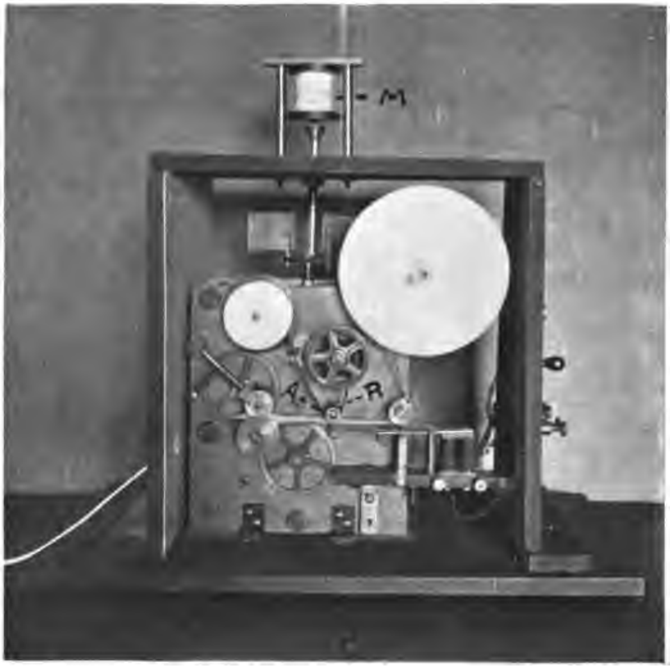

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